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BINAURAL VS. MONAURAL SENSIBILITY OF THE HUMAN EAR TO SMALL DIFFERENCES IN FREQUENCY[1]

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Introduction

The most interesting of the human sense organs are those used in the perception of light and sound, and many investigations have been made on these organs. In audition it is known that the perception of a sound is dependent on pitch, intensity, and the duration of the stimulus. Much of the investigation carried on in this field has been done on pathological ears from the viewpoint of diagnosis, while not so much has been done on the normal ear.

The most important types of investigation made on the normal ear have been on the lowest audible pitch, the highest audible pitch, the minimum intensity required to stimulate audition, the barely perceptible change in intensity as a function of intensity and pitch, and the barely perceptible change in pitch as a function of intensity and pitch. In this last field numerous investigations have been made on the single ear for pitches within the range most used in speech, and for persons of normal hearing. But apparently no exact measurements have been made on a large number of observers of normal hearing at the pitches most used in ordinary speech to determine to what extent the right and left ears of the same person differ in pitch sensibility, or to what extent the single ear differs from the two ears used simultaneously.

[1]From the Ryerson Physical Laboratory, University of Chicago. The author wishes to thank Professor H. G. Gale, under whose direction the investigation was undertaken, for offering many valuable suggestions. He also is indebted to Dr. A. J. Dempster, Dr. F. M. Kannenstine and Dr. V. O. Knudsen.

Knudsen[2] while investigating the sensibility of the single ear to small differences of intensity and frequency obtained evidence which indicated that the right and left ears are nearly the same in pitch sensibility, while the two ears when used simultaneously are better than is either ear when used separately. Knudsen's work was carried out only for a single pitch of 1,000 d.v. (double vibrations, or cycles); and even for this pitch he did not feel that his investigation was sufficiently extensive in scope to warrant a positive conclusion.

In view of this, it was considered desirable to make a careful investigation on the ears of several persons of normal hearing, to see whether the right and left ears have the same resolving power, and to determine whether there really is a difference between one ear and two ears in resolving pitch. It also seemed desirable to carry the investigation of pitch resolution to pitches higher than those used in ordinary speech, which has usually been considered. No investigation has been made in this region, even on the single ear, so far as the writer is aware. (Knudsen[3] has investigated 15 single ears from 12 different persons up to 3,200 d.v.). Information derived from these proposed investigations ought to throw important light on the pitch-differentiating mechanism of the ear.

Problems

The specific problems investigated in this paper are:—

(1) The pitch sensibility, measured inversely by $\Delta N/N$, at varying levels of loudness, first with each ear separately, then with the two ears functioning together, at a constant pitch of 1,600 d.v.

(2) The pitch sensibility, measured inversely by $\Delta N/N$, at a constant level of loudness, first with each ear separately, then with the two ears used simultaneously, for pitches within the range from 64 d.v. to 4,096 d.v.

(3) The pitch sensibility, measured inversely by $\Delta N/N$, at a constant level of loudness, first with each ear separately, then with the two ears used simultaneously, for pitches between 4,000 d.v. and 8,000 d.v.

(4) Data and curves showing the results of the principal investigators with a statement and discussion of the factors affecting the result.

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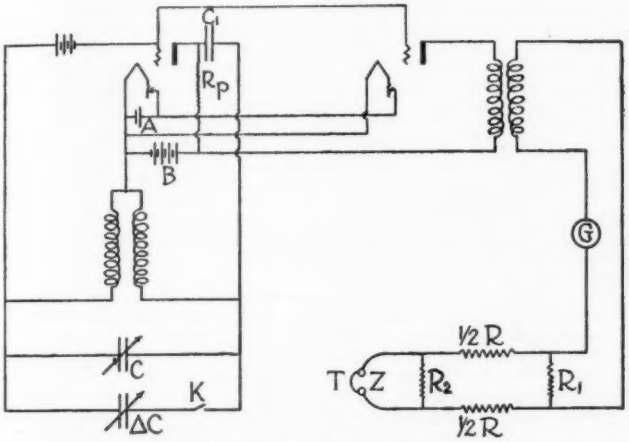
[2] V. O. Knudsen, The Sensibility of the Ear to Small Differences of Intensity and Frequency. *Phys. Rev.*, 2nd Series, 19, 1922, 261 ff.

[3] *Loc. cit.*

Apparatus and Procedure

Apparatus. Fig. 1 shows a diagram of the apparatus which was used for making the investigation. The tones used were produced by a telephone receiver actuated by electrical energy from a vacuum tube oscillator. This method of producing sound by means of transforming electrical energy from a vacuum tube oscillator as a source of alternating currents offers possibilities of pitch control, intensity control, and quality control which are not approached by any other method. The oscillator is capable of producing any pitch within the limits of audibility. Tests made with manometric flames and resonators show that the tones are approximately pure when the plate resistance is kept sufficiently high, provided the currents in



Circuit Diagram

Fig. 1.

the telephone receiver are not too large. Overtones, however, can be introduced by making the plate resistance of the oscillator sufficiently low. The data in this investigation were obtained with approximately pure tones, though the quality of the note does not appreciably affect sensibility, as has been shown by Knudsen.[4]

The constant note within a given range was produced by varying the capacity C which was furnished by a three-dial condenser reading to 1 micro-farad capacity. The ranges of frequency were produced by using different inductance coils. The lower of the two pitches presented successively to the ear was produced by putting a capacity ΔC in parallel with C. This variable capacity ΔC was furnished by an air condenser and a three-dial mica condenser in parallel. By varying ΔC the note of lower frequency can be made to differ from the constant note by any amount desired. This additional capacity ΔC was thrown in and out periodically

[4] *Loc. cit.*

by a motor-controlled mercury switch, K, which was also arranged so that it could be controlled by hand. By varying ΔC the two notes can be brought gradually as close together as desired, or they can be separated gradually as far apart as desired. By varying the speed of the motor the two notes which are presented to the ear can be sounded in rapid succession, or in slow succession. Better repetitions were obtained when the change from one note to the other was about once a second. The duration of each note can be controlled by the amount of mercury in the cup of the mercury switch. These data were obtained when the notes were sounded for the same length of time.

The network of resistances R, R_1, and R_2 permits an accurate adjustment of the intensity to any desired level. R is distributed equally on each side of the line, and it is kept constant at about 5,000 ohms during a test. One side of the line is grounded.

The receivers used were a pair of double phones, varying from a pure resistance of about 3,000 ohms to an impedance of about 90,000 ohms at a frequency of 4,096 d.v. The receivers were screened against electrostatic effects and grounded. Preliminary tests indicated that it would be more desirable to connect the two phones in series and in phase, and all of the data recorded in this investigation were taken with the phones so connected. The intensity in the two phones was equal, as nearly as could be detected. It is essential that the intensity in the two phones be equal so that there is no tendency for the louder tone to mask the weaker one.

When one ear was being tested, the current was cut out of the phone which was placed over the other ear. The impedance used in calculating the intensities was determined for the double phones and for the single phones under the exact conditions of use. When making the tests, the phones were fitted comfortably to the ears of the observer, and care was used to keep the pressure as nearly as possible the same in every case.

The galvanometer, G, measured the total current, I, which flows into the network of resistances. The data for Fig. 2 were taken with a Duddell thermo-galvanometer; the data for Figs. 3, 4 and 5 were taken with a low resistance thermo-couple and a low range milli-ammeter.

Altogether four units of inductance were used in the tuning circuit of the oscillator. For frequencies below 200 d.v. an iron core inductance was used. For frequencies above 200 d.v. wooden core inductances were used. These inductances were closely coupled to give stability of oscillation and output. The output of the circuit shown in the diagram was fairly stable from day to day. The oscillator was calibrated by comparison with standard tuning forks at a constant temperature, by the method of beats. This calibration could have been made from the constants of the circuit, but the former method was considered more accurate as well as more convenient. For frequencies above 200 d.v. the calibration did not vary appreciably over the period of more than a year during which the data were being taken. For the frequencies below 200 d.v., where the iron core inductance was used, wider variations occurred, so that frequent calibrations had to be made for this range.

Procedure. The loudness of a sound, according to Miller[6], is a comparative statement of the strength of the sensation received through the ear, and hence it is impossible to state simply the factors which determine loudness. There is no unit of loudness for measuring sound, so we are dependent on the subjective comparison of our sensations. The ears of different *O*s are of different sensitiveness to sound, and the ears of the same *O* vary in sensitiveness at different pitches. In studying the physical characteristics of sounds, we are, therefore, forced to consider intensity,

[6]D. C. Miller, *The Science of Musical Sounds*, 1916, 53 ff.

not as loudness perceived by the ear, but as determined by the energy of vibration, which varies as the square of the amplitude when the frequency remains constant.

From theoretical considerations Rayleigh says that the acoustical energy given out by a telephone receiver is probably directly proportional to the mechanical energy of the vibrating diaphragm, and therefore to the electrical energy which actuates it. He suggests, however, that this relation needs experimental verification. An experimental test of this relation was made by Knudsen,[6] who found that a linear relation did exist between the acoustical energy put out by the receiver and the electrical energy which actuates it. Since the energy which actuates the receiver is proportional to the acoustical energy output, the intensity, or loudness, of sound produced by a telephone receiver varies as the square of the current in it, provided the impedance remains constant, which it may be expected to do at a given frequency. Hence the current which flows through the telephone receiver is an accurate measure of the loudness, or intensity, of the sound.

From the network of resistances shown in Fig. 1, the total impedance of the network is at once seen to be given by

$$R_T = R_1 [R + R_2Z/(R_2 + Z)]/[R + R_1 + R_2Z/(R_2 + Z)]. \quad [1]$$

The current, I_R, passing through the receiver is given by

$$I_R = R_1R_2I/ [R_2 + Z] [R + R_1 + R_2Z/(R_2 + Z)], \quad [2]$$

where I is the total current flowing through the network, and is measured by the galvanometer, G. The resistance, R, is divided between the two sides of the line, is made rather large, and is kept constant during a test. The current flowing through the receiver is adjusted by varying R_1 and R_2. The threshold current, I_o, is the current which actuates the receiver diaphragm when the note is barely perceptible. In determining this the operator varied R_1 and R_2 until *O* indicated that the note was barely audible. A short-circuiting key is placed across the receivers so that *O* can close it and cut the current out of the receivers, and then open it and let the current flow through again. In this way *O* can be more certain that he has reached his threshold of audition.

When the threshold has been reached, R_1 and R_2 are adjusted to give the required test current, and then K is operated by the motor to give the two pitches. A value is given to ΔC sufficiently large for *O* to hear the two notes distinctly; then the operator gradually decreases ΔC until the two notes merge into one note. This value of ΔC is noted and recorded. Next, ΔC is given a value sufficiently small for *O* to hear only one note, and then it is slowly increased until the observer indicates that he hears two notes. This value of ΔC is noted and recorded. The mean of three independent observations in each direction is taken for the final value of ΔC. If *O* has difficulty in repeating his judgment consistently, the operator sets ΔC at various places near the indicated value and asks the observer to give his judgment as to whether he hears one or two notes, in order that the rhythm of the change from one note to the other may not be so marked. As far as possible *O* is kept from knowing what the operator is doing, in order not to prejudice his judgments. Fatigue effects are minimized by having *O* rest frequently.

Seashore[7] says that one can not hope to get as reliable a test on a large number of individuals selected by chance as upon a single individual. He further says that those who have keen ears for pitch discrimination reveal the physiological limit at once. It is believed that the data here recorded represent the physiological limit because of the care used in selecting the *O*s

[6] *Loc. cit.*

[7] C. E. Seashore, The Measurements of Pitch Discrimination, *Psychol. Mon.* No 53, 1910.

for the tests, and because of the consistency in repetition which they showed. Many of the *O*s were graduate scientific students, trained in making careful observations. Either because they had poor ears or because they were unable to concentrate sufficiently well, it was found that some of those selected could not make consistent repetitions. No data were recorded for these.

The investigation was carried out in rooms comparatively free from disturbing noises. The data for Fig. 2 were taken in a room having double doors and no windows. The data for Fig. 3 were taken partly in an ordinary laboratory at times when the building was comparatively free from noises, and partly in a specially constructed sound-proof booth. The data for Fig. 4 were taken in the specially constructed sound-proof booth.

By the number of beats between a standard tuning fork and the oscillator, frequencies were determined for the oscillator when ΔC was placed in parallel with C. These frequencies were determined for various values of ΔC at each value of the constant frequency at which tests were made. From a knowledge of the constant test frequency, the frequencies of $C + \Delta C$, and ΔC, curves were prepared from which the pitch sensibility could be read for a given value of ΔC.

Results

(1) *Pitch Sensibility as a Function of Intensity at 1,600 d.v.* Fig. 2 shows how the pitch sensibility of the two ears used together compares with that of the single ear for a constant pitch at different levels of loudness. Pitch sensibility, throughout this investigation, is measured inversely by the ratio of the barely perceptible change in pitch, ΔN, to the pitch, N. The curves represent the average values for 10 *O*s of normal hearing. Normal hearing signifies that *O* had never had any serious ear trouble and was not aware of any impairment of hearing. The curve marked "Right Ear" represents the average values for the 10 right ears; the curve marked "Left Ear" represents the average values for the 10 left ears; while the curve marked "Both Ears" represents the average values for the 10 *O*s when the two ears were used simultaneously. The ordinates represent $\Delta N/N$, measuring inversely pitch sensibility as defined above. The abscissae, I_R/I_0, represent the ratio of the test current in the telephone receiver to the threshold current. Values are shown for this ratio varying from 4 to 4,096. Intensities where this ratio was less than 4 were too weak for reliable repetitions, and values of this ratio above 4,096 were so loud as to produce rapid fatigue. At these high intensities the ear was so easily fatigued that it had to be rested often. The pitch used for these observations was 1,600 d.v. Tests made at other frequencies indicate that the curves there are of the same general shape.

The threshold current in the telephone receiver was determined in the manner indicated above, and the ratio of the test current in the telephone receiver to the threshold current was increased by multiples of 4 from 4 to 4,096, by means of calculations made from equation [2]. The threshold current

was determined separately for each ear and for the two ears used together, and the higher intensity values were based on these values.

The curves show that pitch sensibility is essentially the same for the right ear as for the left ear, but that it is greater for the two ears when used together. This difference between the single ear and the two ears is greatest for small intensities and least for high intensities. For medium intensities the difference is practically constant.

From the curves it is seen that the average single ear at 1,600 d.v. can detect a change in pitch of 0.63%, or 10.08 d.v.,

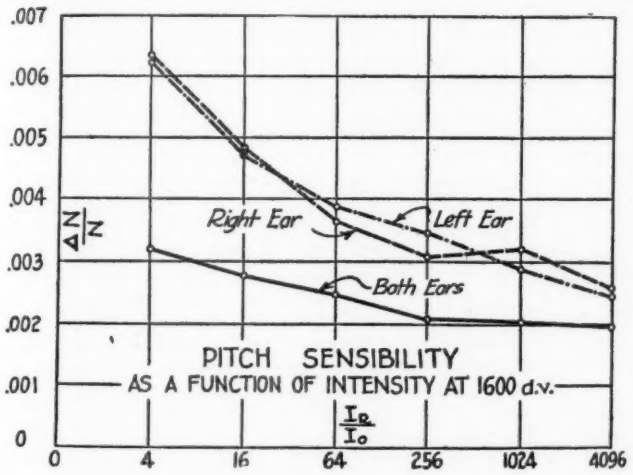


FIG. 2.

when the test current is 4 times the threshold current, while the two ears functioning together can detect a change in pitch of 0.33%, or 5.28 d.v., at the same pitch and current ratio. Likewise the average single ear at 1,600 d.v. can detect a change in pitch of 0.25%, or 4 d.v., when the test current is 4,096 times the threshold current, while the two ears working together can detect a change in pitch of 0.2%, or 3.2 d.v., at the same pitch and current ratio. For intermediate current ratios the differences between the single ear and the two ears lie between these two extreme values.

The results show that at a current 4 times the threshold, two ears resolve pitch 48% better than the single ear, at a current 16 times that of the threshold 41% better, at a current

64 times that of the threshold 34% better, at a current 256 times that of the threshold 36% better, at a current 1,024 times that of the threshold 33% better, and at a current 4,096 times that of the threshold 13% better.

It should be noted that the *O*s for the tests were not selected on the basis of musical training, though some of them had had musical training. They were required to detect merely a change in pitch and not the direction of the change. In this, those who had had no musical training did quite as well as those who had had musical training.

The curves of the individual *O*s are of the same general shape as the average curves which are shown, but some of the individuals showed a sensibility less than the average at certain intensities, and greater than the average at other intensities. The two ears, however, were always better for resolving pitch than one ear, provided there was no marked impairment of hearing in one ear, as sometimes occurs at some particular frequency.

(2) *Pitch Sensibility as a Function of Frequency at Frequencies Ranging from 64 d.v. to 4,096 d.v.* Fig. 3 shows how $\Delta N/N$ changes with frequency from 64 d.v. to 4,096 d.v. at a constant level of loudness. The curves represent the average values for 40 *O*s of normal hearing. The test amplitude of vibration used is kept constant for each pitch, being 100 times the threshold amplitude. The curve marked "Right Ear" represents the average values for the 40 right ears; the curve marked "Left Ear" represents the average values of the 40 left ears; while the curve marked "Both Ears" represents the average values for the 40 *O*s when the two ears are used simultaneously. The right ears, left ears, and the two ears are, of course, those of the same 40 people. The ordinates, $\Delta N/N$, represent inversely, as before, the pitch sensibility. The abscissae represent the pitches at which the tests were made. The test amplitude chosen was sufficiently high to permit accurate decisions, but not high enough to produce serious fatigue. The range of pitch covered is that which is most important in ordinary speech.

The curves show that the two ears are approximately alike in pitch discrimination at a constant level of loudness when used separately. The fact that the pitch sensibility of the left ear is slightly better here than that of the right ear may be regarded as accidental, or it may be due to the fact that the left ear has had more training, since it is the ear used in telephone conversation. This apparently better sensibility of the left ear is noticed only in the lower pitches and lower intensities, which are the most important in telephone conversation. In Knudsen's in-

vestigation, which was referred to in the first part of the paper, the same apparently better sensibility of the left ear was observed. For higher intensities and frequencies the right ear seems to have the better sensibility quite as frequently as the left ear. The two ears when used together are appreciably better at all frequencies and intensities. The difference between the single ear and the two ears is practically constant from 64 d.v. to 4,096 d.v. This difference between the single ear and the two ears is shown for 40 *Os* of normal hearing in the table below, expressed in vibration differences barely audible.

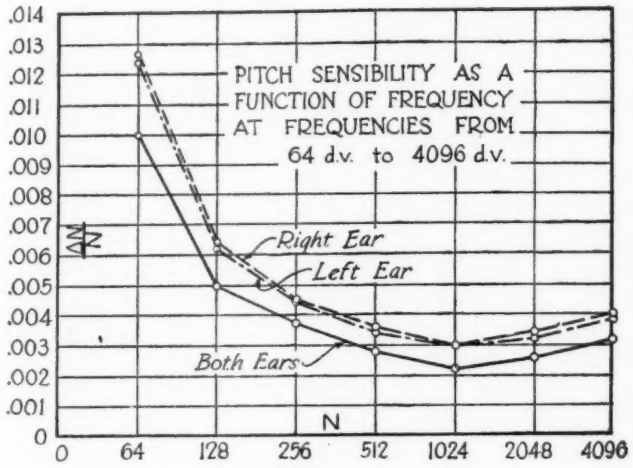


FIG. 3.

TABLE I

N	ΔN_s (Single Ear)	ΔN_B (Both Ears)	$\Delta N_s - \Delta N_B$ (Difference)
64	0.80	0.62	0.18
128	0.81	0.64	0.17
256	1.14	0.97	0.17
512	1.81	1.43	0.38
1024	3.01	2.23	0.78
2048	6.70	5.32	1.38
4096	15.97	12.88	3.09

The table shows the difference between the single ear and the two ears in terms of vibration differences. Column 1 shows the pitches at which the tests were made. Column 2 shows the vibration difference between the two notes when the single ear just resolves them. This means, for instance,

that at a frequency of 64 d.v. the average single ear can distinguish two pitches when they differ by 0.80 d.v. At 4,096 d.v. the average single ear can distinguish two pitches when they differ by 15.97 d.v. For intermediate pitches the barely perceptible differences in frequency lie between these two extreme values. Column 3 shows the vibration differences between the two notes when the two ears can just resolve them. At a frequency of 64 d.v. the two ears of the average person can distinguish two pitches when they differ by 0.62 d.v. At 4,096 the two ears of the average person can distinguish two pitches when they differ by 12.88 d.v. For intermediate frequencies the vibration differences between two notes which can just be resolved by the two ears lie between these two extreme values. Column 4 shows the differences, expressed in vibrations, between the vibration differences resolved by the single ear and those resolved by the two ears. This difference is seen to grow larger as the frequency of the note increases.

The results show that at a frequency of 64 d.v., two ears resolve pitch 23% better than one ear, at a frequency of 128 d.v. 21% better, at a frequency of 256 d.v. 15% better, at a frequency of 512 d.v. 21% better, at a frequency of 1,024 d.v. 26% better, at a frequency of 2,048 d.v. 21% better, and at a frequency of 4,096 d.v. 19% better.

The curves for individuals were of the same general shape as the mean curves, though the curves of certain observers showed greater sensibility than the average at certain frequencies and less sensibility at other frequencies. It was always found, nevertheless, that the observer could distinguish appreciably smaller differences with the two ears than with the single ear, unless the hearing was markedly impaired for one of the ears at some particular frequency, which may happen in the case of persons of apparently normal hearing. This impairment shows up in the form of an abnormally high threshold for the impaired ear. For such persons at these particular frequencies the two ears are no better in resolving pitch than the better of the two ears. It should be noted, however, that an abnormally high threshold for a particular frequency does not mean that the sensibility is higher when the test amplitude is made 100 times that of the threshold. The absolute intensity in this case is much higher, and consequently the impaired ear distinguishes change in pitch as well as the unimpaired ear.

In a different type of investigation Wegel[8] has found that when one ear becomes less sensitive, even though the loss is small, the use of the binaural sense disappears, and after a time is not missed. He also found that for the binaural sense to be utilized the two ears must be very nearly alike. In certain cases of deafness which he has investigated, where one ear is much more impaired than the other, the hearer does not use the binaural sense. In such cases he has fitted to the individual a compensating deaf set so that he hears the sound equally well with both ears. With this compensation the individual manifests a marked pleasure, provided he experiences the binaural sense. Wegel states also that in some cases it takes practice to restore the binaural sense, just as it does to restore the sense of perspective when glasses are fitted to eyes, one of which is defective.

[8] R. L. Wegel, Binaural Aids for the Deaf, *Proc. Nat. Acad. Sci.*, 8, 1922, 155 ff.

The same experience seems to apply in the resolution of pitch. When the threshold amplitude of one ear is several times that of the other for some frequency, the two ears resolve pitch no better than the better of the two ears when the test amplitude for the two ears is set at 100 times the threshold amplitude of the two ears, and when the test amplitude of the better ear is set at 100 times its threshold amplitude. Experiments show that this holds equally well for other test amplitudes. Evidently in this case only the better ear is functioning, even though both ears are in use. The intensity in the impaired ear is not sufficiently great to enable it to aid effectively in pitch discrimination. In the ears tested, this tone deafness in one ear occurred only in a few cases, and then only at some particular frequency. There seems to be no evidence to show to what extent this particular type of tone deafness prevails. Seashore[9] says that tone deafness probably prevails to the same extent as color blindness, though there are no satisfactory statistics on either.

It would prove interesting to fit a compensating set to persons having this tone deafness in one ear at a particular frequency, so that they could hear the pitch with equal intensity in both ears, and then test them for binaural pitch resolution.

The 40 persons tested above ranged in age from 18 to 40, the average age being 25. Of the 40, 9 were women and 31 men. The individual curves showed no detectible variation for sex. The average curve for the 8 oldest *O*s, whose ages averaged 36, showed that they did not resolve pitch quite as well as the younger *O*s. This was noticeable both when the single ear was used and when both ears were used together. The variation of the older *O*s from the younger was so slight, however, that it might well have been due to characteristics of the individuals. So the evidence on this point is not regarded as conclusive.

All of those whose curves were included in the average curves of Fig. 3 were able to repeat their decisions within limits of 10% variation. The test is quite a tax on one's powers of concentration, and it was found that some observers either did not have ears sufficiently good, or else could not concentrate sufficiently well to give reliable repetitions. For these persons no data were recorded.

Twelve of the 40 *O*s had had some musical training varying in amount from 3 months to 6 years. The *O*s were required to detect merely a change in pitch and not the direction of the change, and in this task those who had no musical training did as well as those who had musical training, though the latter could detect the direction of the change somewhat better than the former, as has been found by previous investigators.

(3) *Pitch Sensibility as a Function of Frequency at Frequencies Ranging from 4,000 d.v. to 8,000 d.v.* Fig. 4 shows how pitch sensibility at a constant level of loudness varies with pitch for frequencies ranging from 4,000 d.v. to 8,000 d.v. The amplitude used is 100 times the threshold, as before. The curves

[9] *Loc. cit.*

are the average values for ten persons of normal hearing. Pitches are plotted as abscissae, and $\Delta N/N$'s as ordinates. The curve representing the average values for the right ears is marked "Right Ear;" that for the left ears, "Left Ear," and that for the two ears used together, "Both Ears." The individual curves are of the same general shape as the average curves.

The curves show that the pitch sensibility, varying inversely as $\Delta N/N$, diminishes somewhat rapidly for pitches ranging from 4,000 d.v. to 8,000 d.v. This is true for each ear

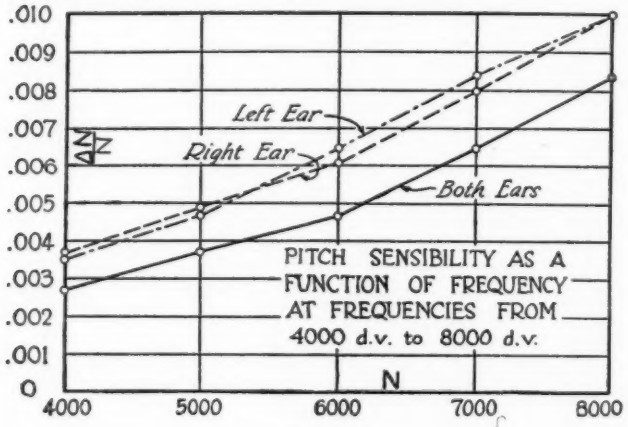


FIG. 4.

used separately, and for the two ears used together. The pitch sensibility, however, is still approximately the same for each ear when used separately, but it is appreciably higher for the two ears. For the single ear $\Delta N/N$ is 0.00365 at 4,000 d.v., and it increases to 0.01 at 8,000 d.v. For the intermediate pitches it lies between these two extreme values. For both ears it increases from 0.00275 at 4,000 d.v. to 0.00840 at 8,000 d.v., while for intermediate pitches it lies between these two extreme values.

For these higher pitches, the inductances and capacitances used are comparatively small, so that the inductances and capacitances in the line become appreciable. In view of this the line was made as short as possible. The motor-controlled mercury key was cut out, and the key was operated by hand. Precaution was taken to reduce inductance and capacitance effects in the line. Any of these effects remaining were taken into account in the calibrations.

At these high frequencies it was thought that there might have been some appreciable change in loudness between the two

notes as they were successively presented to the ear, so that what the observer heard was a combination change in frequency and loudness. So far as could be detected by the ear or by the galvanometer which measured the current flowing into the network, there was no change in intensity. Knudsen's[10] results show that there would have to be a change in current greater than 6% for the change in intensity to be detected at this frequency and level of loudness. Since a change in current of less than 1% could have been detected, it must be concluded that change in intensity is not a matter of importance here.

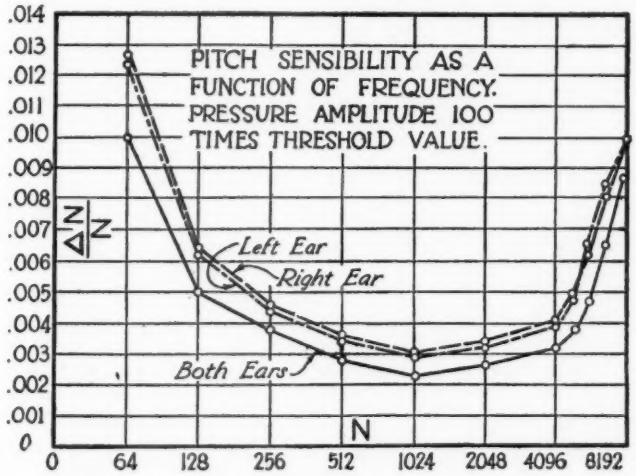


FIG. 5.

Of the 10 persons tested, there were 9 men and one woman. All but one were graduate students in science, trained in making careful observations. The age of the *O*s ranged from 23 to 40, the average age being 31. None of the *O*s had had musical training. The *O*s were required, as before, merely to detect change in pitch, and not to tell the direction of the change. All of the *O*s whose curves are included in the average curves were able to repeat their decisions consistently, though the variation in repeating decisions was somewhat greater for the higher pitches.

(4) *Pitch Sensibility as a Function of Frequency at a Pressure 100 Times the Threshold Value.* Fig. 5 is a combination of Fig. 3 and Fig. 4. This combination gives a picture of how pitch sensibility varies from 64 d.v. to 8,000 d.v.

[10] *Loc. cit.*

Results of Principal Investigators

An examination of the literature on pitch sensibility shows that there is considerable disagreement in the results of the principal investigators. This is due partly to the fact that there was no accurate means of controlling intensity in most of the methods employed, so that what the observer heard was a combination of change in pitch and change in intensity. It is also due in part to the fact that some of the investigators used one ear while others used two ears. The regularity with which the two notes are presented to the ear and the relative time of duration of the two notes affect the results, though to a much less degree than the other factors.

Vance[11] has given a good historical survey of the problem, and has made a valuable contribution to the literature on this subject by his own investigations.

Preyer[12], using vibrating reeds, states that 1/3 d.v. in 500 d.v. and 1/2 d.v. in 1,000 d.v. can always be heard by the best observers. His *Os* could not resolve as two notes 500 d.v. and 500.2 d.v., or 1,000 d.v. and 1000.3 d.v.

Luft,[13] using tuning forks having sliding weights, gives:

N	64 d.v.	256 d.v.	2.048 d.v.
ΔN	0.15	0.23	0.36
ΔN/N	0.0023	0.0009	0.00017

Meyer,[14] using tuning forks with adjustable set screws attached, finds the following:

N	100 d.v.	400 d.v.	1,200 d.v.
ΔN	0.54	0.28	0.69
ΔN/N	0.0054	0.0007	0.00059

Stucker,[15] using tuning forks, a monochord, and a Galton whistle, finds the following as the average for 50 ears:

N	73.4 d.v.	130.5 d.v.	261 d.v.	435 d.v.
ΔN	0.7	1.0	1.3	1.4
ΔN/N	0.0094	0.0074	0.0049	0.0032
N	870 d.v.	1740 d.v.	3132 d.v.	6264 d.v.
ΔN	2.5	7.7	26.7	304
ΔN/N	0.0030	0.0044	0.0086	0.0491

Vance,[16] using tuning forks in front of resonators, obtained the following as the average of 50 ears:

N	64 d.v.	128 d.v.	256 d.v.
ΔN	3.4	1.55	1.50
ΔN/N	0.053	0.012	0.0058
N	512 d.v.	1,024 d.v.	2,048 d.v.
ΔN	1.85	3.35	5.94
ΔN/N	0.0036	0.0033	0.0029

[11] T. F. Vance, Variation in Pitch Discrimination within the Tonal Range, *Psychol. Mon.*, 16, 1914, 115 ff.

[12] W. Preyer, *Die Unterschiedsempfindlichkeit für Tonhöhen*, 1876, 24.

[13] E. Luft, Ueber die Unterschiedsempfindlichkeit für Tonhöhen, *Phil. Studien*, 4, 1888, 511 ff.

[14] M. Meyer, Ueber die Unterschiedsempfindlichkeit für Tonhöhen, *Ztsch. f. Psychol. u. Physiol. d. Sinn.*, 16, 1898, 352 ff.

[15] N. Stucker, Ueber die Unterschiedsempfindlichkeit für Tonhöhen in verschiedenen Tonregionen, *Sitz. Kais. Ak. d. Wiss., Wien, Math. Kl.*, 116, 1907, 367 ff.

[16] *Loc. cit.*

Knudsen,[17] using a telephone receiver actuated by a current from a vacuum tube oscillator, obtained the following as the average for 15 single ears from 12 different people:

N	50 d.v.	100 d.v.	200 d.v.	400 d.v.
ΔN	0.51	0.65	0.90	1.36
ΔN/N	0.0102	0.0065	0.0045	0.0034
N	800 d.v.	1,600 d.v.	3,200 d.v.	
ΔN	2.40	4.80	9.60	
ΔN/N	0.0030	0.0030	0.0030	

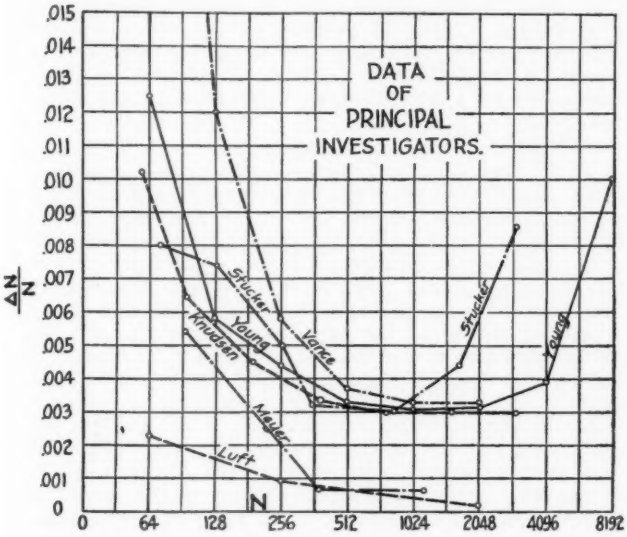


FIG. 6.

These results are more comprehensible when they are shown in graphical form. Fig. 6 shows the results of the 5 principal investigators, together with those of the writer. The pitch sensibility is plotted as ordinates and frequency as abscissae. The curves are marked with the name of the investigator who obtained them. The data for the curve of the writer are the average of 80 single ears from 40 different people between the frequencies of 64 d.v. and 4,096 d.v. Between 4,096 d.v. and 8,000 d.v. the data are for the average of 20 single ears from 10 different people. The curves representing the data given by Stucker, Vance, Knudsen, and the writer have the same general shape and agree remarkably well between the frequencies 512 d.v.

[17] *Loc. cit.*

and 1024 d.v. There is seen to be considerable disagreement for the lower frequencies. For these lower frequencies, the curve representing the author's data agrees most nearly with Knudsen's curve, which is to be expected, since the data for Knudsen's curve and for the writer's curve were obtained by a method which offers better control of intensity, pitch, regularity of change from one note to the other, and duration of sounding the two notes, than was available at the time when the other investigations were made. These two curves agree almost exactly between 512 d.v. and 2,048 d.v. Their variation can be explained as due to characteristic differences in the individuals tested. The curve of the writer should be more representative, in that it is based on the average of a larger number of ears from a larger number of individuals.

Summary

(1) At 1,600 d.v., the pitch sensibility at the same level of loudness is approximately the same for the right and the left ear, but it is appreciably greater for the two ears used together. This characteristic difference between the single ear and the two ears holds as the intensity of the note changes from a current 4 times that of the threshold to a current 4,096 times that of the threshold. The difference between one ear and two ears is somewhat greater for low intensities. For currents ranging from 64 times that of the threshold to 1,024 times that of the threshold, the difference between the single ear and both ears remains practically constant. For greater intensities the difference becomes somewhat less marked. Tests made at other frequencies show that this characteristic difference obtains there.

(2) The pitch sensibility at a constant level of loudness 100 times that of the threshold amplitude is approximately the same for the right and the left ear, but it is greater for the two ears functioning together. This characteristic difference between one ear and the two ears holds throughout the range most important in speech, from 64 d.v. to 4,096 d.v., and is practically constant throughout this range.

(3) At frequencies between 4,000 d.v. and 8,000 d.v., the pitch sensibility at a constant level of loudness, 100 times the threshold amplitude, is approximately the same for the right and the left ear, but it is greater for the two ears functioning together. The pitch sensibility in this range of pitch decreases rather rapidly both for the single ear and for the two ears, and the characteristic difference between the single ear and the two ears is somewhat greater.

(4) It should be especially noted that in the case of the single ear, and for the frequencies most used in speech, the investigation gives information on a larger number of normal ears than has been obtained before by a method in which pitch,

quality, intensity, regularity of change from one note to the other, and relative duration of the two notes could be carefully controlled. In addition, the investigation has been carried to pitches higher than those used before, and information is obtained which shows the difference between the sensibility of the single ear and that of the two ears, for pitches ranging from 64 d.v. to 8000 d.v.

There is no generally accepted theory as to how the ear differentiates pitches, but any comprehensive theory of the pitch-differentiating mechanism of the human ear must include an explanation of this characteristic difference between one and two ears.

EXPERIENCE AND VISUAL PERCEPTION[1]

By Charles A. Dickinson

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While working on an experiment which employed in its technique, tapping while reading, and responding to a stimulus word while adding digits, certain mental experiences became evident, which were unable to be satisfactorily accounted for. The scrape of the kymograph and the moving of the point tracing on the smoked paper appeared, at times, to be present without meaning. Several attempts were made to place this kind of experience under rigid control with the result that the visual field was finally selected. The problem resolved itself into the present experiment on Experience and Perception.

Apparatus. The apparatus consisted of a tachistoscope of the pendulum disk type, a postcard projector and a ground glass. Two 150-watt, 110-volt, nitrogen lamps were used as illumination for the projection of the stimulus field. The ground glass was placed on a frame which was firmly clamped to a table; the frame of the tachistoscope was also firmly clamped to the table. The projectoscope was placed on a box on the table so that the stimulus field centered evenly on the ground glass, that is, so that no

[1]From a dissertation presented to Clark University for the degree of Doctor of Philosophy. I wish, here, to acknowledge my obligation to the late Professor Edmund C. Sanford, of Clark University, under whose guidance the experimentation was carried on; to Professor John P. Nafe, of Clark University, for many valuable suggestions in arranging the data; and to Dr. L. B. Hoisington, of Cornell University, for many further suggestions in rearranging the material for publication. I wish also to acknowledge my obligation to Professor E. B. Titchener for the privilege of carrying on further experimentation in the Cornell laboratory, with trained observers, the results of which lay the basis for further conclusions as well as substantiating the data herein presented.

one-sided effect would act as a distraction to the *O*s. The disk of the tachistoscope was 13.8 cm. from the lens of the projectoscope, and the ground glass was 94 cm. from the disk. The length of exposure, measured by the tuning fork method, was 64σ.

The *O*s sat at a table 1.2 m. from the ground glass. The illumination of the room was controlled by black draw-curtains, sufficient illumination being admitted to prevent the occurrence of a definite after-image on the ground glass, and to allow for the writing of the introspections. The whole right side of the apparatus, from the tachistoscope to a point beyond the position of the *O*s, was bordered with black cardboard in order to prevent the appearance of shadows on the ground glass from the illumination admitted through the windows. On the ground glass was pasted a fixation spot of black paper, at a height adequate to allow the *O*'s line of regard to be as nearly as possible in the horizontal plane.

Observers. The observers used in these experiments were Miss E. V. Howell (H), a graduate student in the department of psychology; F. A. Geldard (G), an advanced senior taking graduate subjects; R. S. Erickson (E) and K. E. Pohlman (P), seniors; F. J. Loungway (L) and H. P. Stedman (S), juniors and the author (D). D and H had had considerable observational experience; the remainder of the *O*s had previously observed in another problem for the writer.

Instructions.

Sec. 1, Series 1. "A field of nine letters will be exposed on the ground glass before you; when this field is exposed, attend to the field in general, not to any particular section of it; then immediately following the exposure describe, orally, what you perceive. A 'ready' signal will be given, followed—with a 2 sec. interval between—by a 'now' signal, when the trigger of the tachistoscope will be released and the stimulus field exposed. Fixate visually the center dot and have no eye-movement."

Sec. 1, Series 2. "A field of nine letters will be exposed on the ground glass before you; when this field is exposed you are to name the center letter and give the location of one other letter which will be called by *E* immediately following the exposure of the stimulus field; anything you may wish to add to this will be acceptable. A 'ready' signal will be given . . ." and so on, as in Series 1.

Sec. 1, Series 3. "A field of nine letters will be exposed on the ground glass before you; when this field is exposed you are to name the center letter and give the location of another letter which will be called by *E* just before the 'now' signal; anything you may wish to add to this will be acceptable. A 'ready' signal will be given . . .," and so on as in Series 1.

Sec. 1, Series 4. "A field of nine letters will be exposed on the ground glass before you; when this field is exposed you are to name the center letter and another letter, the location of which will be called by *E* immediately before the 'now' signal; anything you may wish to add will be acceptable" . . . and so on, as in Series 1.

Sec. 1, Series 5. "A field of nine letters will be exposed on the ground glass before you; when this field is exposed you are to name the center letter and another letter which will be called, with its location, by *E* immediately before the 'now' signal; anything you may wish to add will be acceptable" . . . and so on, as in Series 1.

With this set of instructions *E* indicated on the ground glass, to each *O*, the following locations to be referred to as such at all times by both *E* and *O*; top row, left to right, 'upper left,' 'upper' and 'upper right;' center row, left to right, 'left,' 'center' and 'right;' lower row, left to right, 'lower left,' 'lower' and 'lower right.'

Sec. 2, Series 1. "A field of two playing cards will be exposed on the ground glass before you; immediately following you will reproduce, orally, what is called for. You are to give color, suite, number of spots and location of the card, whether right or left. The stimulus field will be exposed successively until you have reproduced all the items correctly and then one other exposure will be given for the purpose of verifying the field as the cards previously reproduced. Note: red diamonds and black diamonds, red hearts and black hearts, black spades and red spades and black clubs and red clubs will be included. A 'ready' signal will be given, followed, with an interval of 2 sec. between, by a 'now' signal, when the trigger of the tachistoscope will be released and the stimulus field exposed. Fixate visually the center dot and have no eye-movement."

Sec. 2, Series 2. "A field of three playing cards will be exposed on the ground glass before you; immediately following you will reproduce, orally, what is called for. You are to give color, suite, number of spots and the location of the card, whether left, center or right." And so on, as in Series 1.

Sec. 2, Series 3. "A field of four playing cards will be exposed on the ground glass before you; immediately following you will reproduce, orally, what is called for. You are to give color, suite, number of spots and location of the card, whether upper left or upper right, or lower left or lower right." And so on, as in Series 1.

Stimuli. In the first section of the experiment the stimuli were black letters printed on white paper, which was pasted on blue-ruled coördinate paper so as to form a field of nine letters in rows of three and evenly spaced. The blue lines, horizontal and perpendicular, gave the appearance of each letter being enclosed in a square. When this field was projected on the ground glass, these squares, in which the letters were centered, were 9.5 cm., the letters being 8.9 cm. high and the whole field occupying a space of 28.6 cm. square.

In the second section of the experiment a field consisting of playing cards was exposed: a 2-card series with the cards placed side by side; a 3-card series with the cards placed side by side; and a 4-card series with two cards above and two directly below. Black diamonds and hearts, red spades and clubs, as well as the usual combinations, were included. When the field was projected on the ground glass, the size of a single card was 8.9 x 12 cm.

Procedure. The first section of the experiment has five series, each carrying its individual set of instructions. Twenty-five exposures were given in each period, but in order to diminish the stereotyping of observational habits, the series were divided, five exposures from each being presented in an observational period. Approximately one hour was required for 25 exposures; the *O*s each carried 3 periods a week. They gave their reports orally, *E* writing them as they were given. When *E* was not sure just what the *O*s meant, they were asked to describe further or more in detail. D acted as *E* for all the other *O*s; the late Professor E. C. Sanford acted as *E* for D. With the exception of D, the *O*s were without knowledge of the problem.

Section 2 embodies three series. Here every series was carried through before another was begun. Each *O*, as in the first section, observed for approximately one hour 3 times a week.

In Section 1, 875 observations were taken and in section 2, 504 observations—a total of 1379 observations.

In discussing these data we should constantly bear in mind that our *O*s were not trained to describe technically, but we should also bear in mind the fact that one may be able to observe carefully and accurately

even when unable to describe technically. We should remember, too, that our *Os* were instructed that a field of nine letters or a field of playing cards, embodying certain definite characters, would be presented. Instructions of this nature naturally set up an *Aufgabe* to report experience in terms of letters and playing cards, but not necessarily with the implication of logical meaning. In fact, when reporting in such terms, the *Os* sometimes explicitly intimated that logical meaning was not present; that, at the initial moment of the exposure, they did not really see letters as such but as visual forms.

Experience as Visual Pattern

A significant fact, brought out by the introspective reports in relation to experience at this level, is that experience here is a mere 'thereness,' clear in contour and in detail of illumination but lacking in logical meaning. This 'thereness' is a totality of the whole field exposed; it is a flat clearness. We present data in support of this fact.

D. "Bare perception of the field with the letters perceived as just bare letters, then center as *H* with inner speech. There appear to be two different processes here. In the first bare perception the process is more passive, then, later, there is a definite attentional focusing on some one letter, with the falling away of all the rest. The letter is now clear in another way; seems to stand out from the background and to carry with it a good deal more implication; more definiteness, *e.g.* as a letter *H*."

D. "The same type of bare perception of the letters, then *C* as center and with inner speech; same fading of the field with the naming of *C*. *C* seems like a raised up letter; comes to prominence in this manner and then fades away. There is always location, definite, of all letters in the bare perception; the location is more definite and better retained than the letters. The background seems to be the most definite in the bare perception."

L. "I saw the whole nine and yet I did not see the actual letters immediately; then I tried to see certain letters, and when I looked at these, [attended to them] the rest disappeared."

L. "Saw nine letters, yet when I see the nine letters I don't know what they are, but when I looked at them I saw *N J B*, and then the field had disappeared. I repeated these to myself. If in the first flash I could quickly run over the whole nine to myself I could give them all."

L. "I don't think I see any more the last flash than the first, but I repeat them to myself before the last flash, and then they mean definite cards." (From playing card series.)

P. "As a matter of fact I see the whole field first; saw the composite group easily and the top and bottom disappeared. The letters I gave you were raised, forward, raised."

P. "I observe the whole field first and then memorize as fast as possible."

G. "In the first flash all the details are clear, but only in the last flash are they like a verification; just a thereness in the first flash; the last flash I'm satisfied I know them." (Report on verification in the playing series.)

D. "The process of immediate bare perception of the field appears to be a regular one, then some letters are reacted to and the others drop out."

S. "A general field of nine letters, but I only seem to recognize three or four out of the field. The ones I recognized are the ones I gave. I got the center letter in the process of looking for the letter called."

S. "I first see the field and then I go to the center and the center row appears to be much easier to get than the rest; and then I go wherever I can, usually to the lower row, so that the upper line is more or less shut out." (Report given after five exposures of the stimulus field.)

The foregoing reports point to the fact that experience at this level is a phase in development prior to the accrual of logical meaning. Here the whole field appears as a flat visual pattern with no further implication, but transformation from this level is exceedingly rapid, so that almost immediately we progress to another level.

Experience and Generic Object

At this further development experience is represented at the level of generic object, and the field has here taken on a different appearance. In place of being entirely flat, parts of it have begun to stand out slightly in relief; they have begun to rise up out of the general field; they have become differentiated in the total field unity, previously undifferentiated except as visual pattern. These forms have taken on meaning, not definite and specific, but generic meaning; meaning merely as object. They have become object forms not yet specifically labeled. Again we present our evidence.

D. "Bare perception of the whole field, the edges not quite so clear as the rest of it. In this first perception all the letters seemed to come almost to the point where they could be reproduced, but then *L* was named and the rest fell away."

H. "I saw distinct squares with dark letters, which stood out clear, not exactly in relief, but the edges clean-cut, which indicates that they are certain letters."

G. "The field perceived distinctly as letters. I can not reproduce any more because I did not go through the process of repeating them to myself while trying to observe the general field."

L. "I saw more letters; the reason I could tell these to you was because I repeated them to myself first, in that order."

E. "The field as a whole was clear. The letters recalled popped out. Once I had six letters, but some disappeared with the rest of the field. I find that immediately after the exposure I repeat the letters I give you, to myself, as I locate them."

P. "I was aware of the general field, with letters as definite letters, but had not time to memorize them all. I got the general field, then the center, then the top and to either side, but not to the bottom."

D. "Bare perception of the field; more letters with letter value than reported. *X* named, then *Y* named, and then shift back again to *X* for retentional reinforcement, then further exploration of the field, but no more letters were retained."

P. "The two left receded back and to the left, and the others not secured simply receded back and out of sight. Those I reproduced I said to myself, either in series or as single letters."

S. "Saw lower right before *D*, but gave *D* first as I could not recall the name of lower right at the time." [S gave lower right as *L*, but it should have been *E*.]

S. "I get the center and then try to recognize the letter called, and if I get that also I get some in the immediate surrounding. It seems to be easier if the letter is at the left of the line. When I get a word like *M UD*, it is easier to get the letters in it and also others."

H. "I see the four cards on the field, and sometimes I recognize them as they go by, but don't see them long enough to fixate them." (From the playing card series.)

From the level of generic object there is again a quick passage to the level of specific object.

Experience and Specific Object

Here a definite label has accrued, the forms have taken on a stability which previously they lacked. They now have the characteristics of a specific letter, with the implication of the logical meaning of that letter. Visually they pop out, or rise up, or come forward markedly toward the observer. Simultaneously with this coming forward the rest of the field recedes away from the observer or fades out. The following reports are characteristic.

L. "The letters in the field that I gave were outstanding, I could tell what the other letters in the field were, but the moment I tried to recall them it was impossible."

D. "The field was perceived in bare perception; it seemed as if all the letters were letters, then there was a continuation of attention on *X*, which stood out very clearly as the center letter in the center square. There was a gradual but very quick sharpening of *X*, with a corresponding dropping away of the rest."

D. "Perception of the field as in the previous exposure, but letters not quite so plain as last time; the *M* as before, from that flatness to a very definite clearness as letter *M*; stood out from the rest of the field; center with background still present."

P. "I see all the letters and then find the letters called for if the field remains long enough. The field fades and the others come forward; the ones I don't get recede out and sideways."

H. "I got the center letter and then I looked at the general field and recognized the letter called for; that and the center letter stood out as a pair, and the rest faded out." *E.* "What do you mean by 'fading out'?" H. "As I get the letter called or the center letter, the rest fade out and the letter I get comes forward."

G. "*B* seems very, very solid, concrete, definite shades of light in back of it, a halo around the letter; it appears to be set off, set out the same as a sign-painter would set it out by putting shades of paint behind it." [*B* was the letter called.]

S. "The field in general was clear. I saw, I should say, about seven letters as distinct letters. The ones I gave seemed to swell right out toward my eyes and then recede after the exposure."

S. "As the letters were called they seemed to come forward into certain positions."

S. "All the letters were distinct as letters; those I remember seem to be pushed out toward me and then drawn right back again."

H. "I seemed to get *B* definitely, and all the rest were equal until you called *O*, and then *O* stood out and the rest of the field disappeared."

E. "Letters *W E G U* in that order, left to right; I saw other letters, but they were hazy and did not stand out." *E.* "What do you mean by 'standing out'? E. "They appeared brighter and seemed to come right out toward my eyes."

P. "I see all the letters and then find the letters called for; the field fades out and they come forward."

This process of accrual to experience, giving it a name, tabulating it as specific object, requires a shift from one part of

the field to another; logical meaning accrues to one item at a time or to a group of items as a unit; it is a successive and not a simultaneous process.

Naming as Specific Object a Successive Process.

G. "Shift of attention from center to the others in the middle row and the rest of the field had not receded, then a shift to the top left and lower left, and then the others had disappeared."

P. "Saw the top fading; got center and then went to left and right; said them to myself."

P. "The process is from center to general field and then to the position called; center row and upper row and not to the lower row until after."

D. "Center first, then shift to *A*, then shift back to center to reinforce *B*, shift to right and *S*, and then to *O*.

Specific Naming and Changes in Contour and Saturation. In addition to the popping out or coming forward of letters as they are specifically named, there also appears to be a decided change in saturation and contour.

E. "As before, the letters I recalled are distinct and the surrounding letters are of a less black color; they seem to get cut right off."

E. "I can distinguish all the letters in the general field as letters; I can remember that in certain places were round letters and in others were straight line letters. The letters I recall are very clear, that is, the black is very black and the shapes are clear and clean-cut. There is a kind of red line very clear around them; they appear like what is called in shadows 'umbra' and 'penumbra'; the outstanding ones are like the dark shadows, the 'umbra.'"

The *O*s often mentioned that between the naming of two letters there was, as it were, a slipping back to the generic level, and at times to the level of visual pattern.

H. "Center to upper right, then across and *E M S* named, then to the general field, that is, I see the general field twice, after seeing the center and the location called, and then to the lower row and then to the general field again."

L. "This time I saw the field as I was trying to get from the location to the center letter."

Recognition as a Fitting of Sense Data and Existing Imagery. Under the instructions for verification, recognition appears to be the fitting of sense data and existing imagery. This was brought out in a number of ways.

L. "Did not see *W*; lower was *X*; did not see all the letters so well as in the first exposure. If not called upon to get certain letters I think I could probably get them all. It is a process of elimination, because I look at another just to see if it is something which is not called." [Reference to the previous exposure by L is reference to the exposures under the first set of instructions.]

L. "Looked everywhere until I got *R* last. I can't tell what letters I saw. *K* was somewhere; have now forgotten the center."

S. "Order of letters *O B S T D Y*; if I had not been told that *D* was there, I should never have recognized it." [In this case *O* thought that *E*

had called the letter *D*, whereas *E* called *T*. *O*, however, reproduced *D*, locating it at the location of *T* and misplacing *T*. *D* was nowhere on the field exposed.]

Color has Advantage in the Naming Process. The reports on the playing card fields exposed, some of which have already been included, coincide with the reports from the letter field series; the playing card series is somewhat more complex, but in the main exhibits similar experimental characteristics. There are additional items, *e.g.* the playing card charts show that color appears to be almost immediate in perception, it is reported constantly when other factors are missed, that is, when neither suite nor number of spots can be stated at all, and it is, in most instances, reported correctly; occasionally its position on the field may be wrongly given yet the color correct. It appears at an advantage in the naming process. We find also that "imaginal overlay" enters in. Other items, too, appear, but we shall state them later in the general discussion of the material.

Discussion

We have submitted introspective data which appear to demonstrate that, in visual perception, there are three main levels, not separate and distinct, but in the nature of a progression, one level developing to another. At these levels we have indicated the nature of 'experience' as visual pattern, as generic object and as specific object; we realize, however, that there is no such thing as level, separate and distinct, although there do appear to be momentary pauses or 'slackenings-up' of experience, in its temporal course, from the initial moment of the exposure of the stimulus field, through the process of the accrual of specific meaning to certain items in the field, to its termination. Many of our reports bear upon this point. Two will suffice to illustrate here.

D. "The field was perceived in bare perception; it seemed as if all the letters were letters, then there was a continuation of attention on *X*, which then stood out very clearly as the center letter in the center square. There was a gradual but very quick sharpening of *X*, with a corresponding dropping away of the rest." (From the letter series.)

L. "When the flash appeared I saw perfectly clearly four cards of different suites and different colors but however plain it may be it is a flat vision inasmuch as it appears to carry no particular meaning; then I habitually attend to the top and the bottom. I am still aware of color, but the moment I get the top as actual cards the bottom is gone, and then I am just seeing two cards at the top; but even then, when I attend to the top row, the process appears to be attending to one and then to the other. In the succesive exposures I really attend to one card at a time, which I definitely determine beforehand, and find it is much easier to take across instead of up and down. When I verify the last exposure I really repeat the cards to myself before the flash comes on, that is, the cards I have seen, and they have fairly definite visual image; if it is not the same, I am aware of the difference. If I find a difference, *e.g.* suppose I say to myself a card

which is not really on the exposure, then I cannot verify it like the others, but must actually attend to it again and get it anew. I always begin with the card I am least sure of because I can easily get the other three." (From the playing card section, series 3.)

Experience at the initial level is merely a flatness, a 'thereness' of visual pattern which carries a wealth of detail of contour and illumination. No one part has begun to stand out from any other part; parts do not exist, the field is a totality and hangs momentarily as such. G states this quite definitely when he reports:

"In the first flash all the details are clear, but only in the last flash are they like a verification; just a thereness in the first flash; the last flash I'm satisfied I know them." (Playing card section.)

D also reports in this connection:

"The process appears to be an immediate bare perception of the field, then a slight reaction to certain letters with a view to establishing them or carrying them over until they can be definitely attended to, then an attentional exploration from center toward the left, up toward the top of the field and over toward the right if the letter called is not located, then back toward the center to reinforce it; some kind of kinaesthesis with certain letters in the course of this attentional exploration." (A general introspection after five exposures.)

The rise from this flat visual pattern to the level of generic object is very quick; the letters become letters and the playing cards become playing cards, but not yet individually named letters or cards. This change shows itself in a very slight coming to prominence of the letters or cards, with a corresponding change in definiteness of contour; the letters are now letters in a field, and the cards are cards in a field, with letter and card contour and not merely contour of visual pattern. Of this H reports:

"I saw distinct squares with dark letters which stood out clear, not exactly in relief, but the edges clean cut, which indicates that they are certain letters."

P states this from another angle:

"Since I saw *MUD* and *H I K* across the center row I always try to put a meaning to the rows of letters. I said the lower row as *EMPTY*, but did not actually observe them that way."

In the first case most of the field was, apparently, taken in at once; in the second case one row was taken as a unit and the letters grouped as a word. Here the letters were *N T Y*, but P reproduced them as *M T Y*. What P really did was to name the three letters as a word unit, as they were visualized, and later to name the letters as individual letters from vocimotor-auditory imagery of the word 'empty.' His imagery misled him with the

result that an *M* was reproduced when it should have been an *N*. When H speaks of the edges of the letters being clean cut, thereby indicating that there are certain letters there, it is evident that differences in letter contour are noted, but the letters are not yet named as specific letters; they are letter, generic.

At the level of specific object certain characteristics appear to be much more concrete than at the previous level. The letters and the cards seem to actually come forward as they are specifically named. Different observers have reported this in different ways.

H. "I got the center letter and then I looked at the general field and recognized the letter called for; that and the center letter stood out as a pair and the rest faded out." *E*. "What do you mean by 'fading out'?" H. "As I get the letter called or the center letter, the rest fade out and the letter I get comes forward."

G. "*B* seems very, very solid, concrete, definite shades of light in back of it, a halo around the letter; it appears to be set off, set out the same as a sign-painter would set it out by putting shades of paint behind it."

S. "The field in general was very clear. I saw, I should say, about seven letters as distinct letters. The ones I gave seemed to swell right out toward my eyes and then recede after the exposure."

From these reports it is evident that, with the specific naming of letters, they appear to stand out or come forward, while the letters not named appear to sink back or recede. Sometimes these letters continue to recede backward until they have entirely disappeared, producing a two-level effect as they go, and sometimes they seem to hang in the balance before they disappear, giving to this two-level effect a certain appearance of stability. This appears to parallel the two levels postulated by Dallenbach.[2] We corroborate Dallenbach's facts almost in entirety. Our data also agree, in part, with those of Oberly.[3]

Again we find, as experience develops to this level, that there is a very noticeable change in color saturation and brightness and in the texture of experience. From E we have the following:

"I can distinguish all the letters in the general field as letters; I can remember that in certain places were round letters and in others were straight line letters. The letters I recall are very clear and clean cut. There is a kind of red line very clear around them; they appear like what is called in shadows 'umbra' and 'penumbra'; the outstanding ones are like the dark shadows, the 'umbra.' "

"As before, the letters I recalled are distinct and the surrounding letters in the field are of a less black color; they seem to get cut right off."

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[2]K. M. Dallenbach, Attributive *vs.* Cognitive Clearness, *Jour. Exp. Psychol.*, 3, 1920, 183-230.

[3]H. S. Oberly, The Range of Visual Attention, Cognition and Apprehension, this JOURNAL, 35, 1924, 332-352.

This change from a loose, soft, shadowy experience to one carrying more of a hardness of surface and a greater saturation appears to be directly correlated with the popping out, or the rising up, of the letters or playing cards as they are definitely tagged with specific, or logical, meaning. If, for example, the form *G* is being specifically labelled as *G*, with the full implication of the letter *G*, there is, visually, a coming forward of the form with a simultaneous hardening or solidifying of the texture, and an increase of brightness and saturation.

In this process of ascription of specific meaning the different items in the field are dealt with individually and successively. At times several items appear to begin to rise up simultaneously but these items are then named as a unit or one item becomes focal to the exclusion of the rest of the field. The reports are emphatic in this respect.

The rest of the letters in the field, that is, the letters not undergoing the process of naming, sink back, or fade out, or are cut right off, and the letter named remains momentarily after the rest have disappeared.

Under the second and third sets of instructions, especially, a very important and interesting fact becomes evident. The letter called sets up imagery and this imagery is in some manner compared or fitted to the letter in the field or the letter in the field is fitted to the imagery set up. Under the second set of instructions there is, as it were, a hunting in the memory after-image for the letter called; if the imagery set up by the calling of the letter fits, then there is recognition of that letter as the letter required, and the location is noted.

S. "I get the center and then get the letter called and any other letters I can, but when I get the letter called there is a tendency to stop, so the number of letters given probably depends on the getting of the letter called; but I want to get the letter called, and so slip over them quickly, and when I get it the others fade away."

L. "I repeated the cards to myself before the exposure and had visual imagery, but I was not sure of the fourth; when the field was exposed, the three I repeated to myself were recognized, but I really had to attend to the fourth once again as it went by." (Playing card series.)

L. "Recognized, a sweep of attention, but almost like the first flash, because I did not definitely repeat them to myself beforehand." (Playing card series.)

At another time L said: "I fit what is there to the visual image; if it is not the same, I am aware of the difference."

S. "I did not look at the letters to be able to recall them, except the center and the one called. I just looked at the letters to make sure that they were not the ones I wanted."

In this fitting of imagery, the field appears just to wrinkle at the place of fitting, the letters dwelt upon, in the attentional exploration of the field rise up ever so little; they do not reach

even the level of generic object, but are merely matched against the existing imagery. The reports suggest that, as a rule, they are recognized with no more implication than being either the same as or different from the imagery set up by the calling of the letter. Sometimes, because of a certain peculiarity resulting from a combination of visual contour and illumination, a contrast effect appears, and, as it were, holds the observer momentarily at that position, and this momentary pause is sufficient to raise that item definitely out of the flatness of visual pattern to the level of generic object, and at times even to the level of specific object.

Imagery of one letter may persist and be reported in place of another letter in another location. Here is such a case.

D. "First of all *I* was perceived as not being *H*, with a clearness which, perhaps, was just a familiarity feel that it was not *H*, then *I* was named and it seemed actually to stand out as if raised up; shift back to *W* and very clear; shift to upper left and then to *J*. When *I* was not *H* there seemed to be a lack of balance which belongs to *H*, in contour and saturation; *I*'s contour and saturation are different. This was the first factor in recognizing that *I* was not *H*; this is of the same type as locating a letter: when you call *H* it makes a mental set for that letter which embodies imagery for *H*, which is expected to be shown on the ground glass."

E[4] had injected a series with D in which occasionally a wrong letter was called, *e.g.* in place of calling the letter *P*, which would show in the lower right corner of the field, *E* would call the letter as *R*, and D was then to give his introspection on the observation. D was therefore alert for false letters and here thought that one had been called. We may note that, in this instance, the upper left position and the letter *H* are called, that is, *H* is in the upper left. D's habitual shift is from center to left before going to upper left, therefore it is natural that the letter *I* should be seen before the letter *H*. This, apparently, is what happened: D visualized the letter *I*, and carried *I*'s imagery up to the position of *H*, which made it appear as though *E* had called a false letter. It is easily discerned from the report that D could not make image *I* fit the visual pattern of *H*, and immediately named *I*, and then shifted to the letter *W*. With the naming, the letter *I* stood out to prominence, and *H* was blotted out, but there is little doubt that if D had remained at the upper left position *I* would very soon have resolved itself into the letter *H*. D noted the difference in balance of the two letters, but evidently was unable to inhibit the naming of *I*, and when to the letter in the upper left position the name had accrued, the *Aufgabe* was fulfilled, and D passed on.

If we turn to the playing card section we find similar results. The field, first of all, is a totality of flat visual pattern, which then progresses through the level of generic object to that of specific object. H reports:

"Had a sweep of attention from left to right and just barely recognized the right hand card." Again: "A very short affair, but a complete flash of clearness, details all there, a sort of flatness to it and then the two upper cards became prominent."

L has also stated: "When the flash appeared I saw perfectly clearly four cards of different suites and different colors, but however plain it may

[4] *E* here used to designate the experimenter.

be it is a flat vision, inasmuch as it appears to carry no particular meaning; then I habitually attend to the top and the bottom. I am still aware of color, but, the moment I get the top as actual cards, the bottom is gone, and then I am just seeing two cards at the top; but even then, when I attend to the top row, the process appears to be attending to one and then to the other."

With the successive exposures of the playing card stimulus field several other factors manifest themselves. In the first place, color appears to be almost immediate in the perception; It appears to have an advantage in the naming process; coupled with location, it is stated with certainty many times where contour and number of spots are stated with uncertainty or are not stated at all. Again, we find the fact of "Imaginal Overlay."[5] For a number of exposures an *O* sometimes reports, *e.g.* a red spade as a black spade, and then, when he eventually reports it as red, says that he has seen it as red all the time.

Sometimes an item is read into the perception which does not exist on the exposure but which materially alters the meaning of the perception.

For five consecutive exposures G named a *7* of red hearts as *8* of red hearts, finally reporting that he really had seen a *7* each time, but that he had added a spot and reported an eight. He said: "I saw a spot in the middle and assumed another one at the top although I really didn't see it." This addition was due to the fact, he said, that the three spots were at the bottom of the card instead of at the top. G is a card player and when playing always holds his cards so that the seven, for instance, will be right side up, that is, the card held so that the extra spot is at the upper and not at the lower end of the card. When the extra spot is at the lower end the card is, for him, upside down; so, although visualizing the seven spots, and no more, he added one in naming the card and reproduced it as an eight.

Four of the *O*s reported what appears to be a very quick reflective process, while attempting to give specific meaning to an item in the field exposed. This was more noticeable in the playing card series than in the letter series and was on several occasions almost as immediate as the transition to generic level. It evidently had little effect on the speed of reproduction, although on one or two occasions the *O*s hesitated slightly, as a result, in beginning their report. It usually occurred in this manner: when the *O* had reproduced, *e.g.* a black heart consecutively for several exposures, or if the *O* was not quite sure whether, in the first exposure, the black heart was really a heart or a spade, instead of stating immediately what the exposure appeared to be, a reflective factor entered in and the *O* began to 'figure' what to name that particular item. In the case of this doubt about the heart in the first exposure, the process was more in the nature of a definite or direct judgment, but in the

[5]G. K. Adams, An Experimental Study of Memory Color and Related Phenomena, this JOURNAL, 34, 1923, 359-407.

case where the suite is named a heart for a number of consecutive exposures, the same field still being presented, it differed somewhat from the nature of a direct judgment; in the final analysis a judgment was formed, and formed very quickly, but in the course of the formation of the judgment the *O*'s mental experience appeared to be, relatively speaking, considerably prolonged and of a reflective nature.

When P hesitated just a trifle in making his report, *E* asked: "Why did you hesitate then?" P replied: "I have been stuck on this card four times now and this time I discovered I was figuring what it should be; very quick, but it was figuring just the same." D also reported: "I notice that there is a secondary process enters in sometimes when I am not quite sure of a card for several exposures; it is so fast that sometimes I can hardly catch it; it appears to take place while the field is still there; like an attempt to juggle the suite instead of stating what appears to be there, when I am not sure." *E*. "Are you quite sure that it is not embodied in your *Aufgabe*?' D. "Quite sure."

This may, perhaps, be considered in the nature of a judgment, as stated by Titchener[6]—an attentional alternation with a certain element of conflict entering in; and there appears to be little doubt that this kind of thing enters considerably into perception. Interest concerning it lies in the speed with which it takes place and its effect on the specific name which accrues to the item in question.

Two additional facts, which may be of importance, have been stated without adequate description; they receive here merely a casual mention. D reports: "In the first perception all the letters, except perhaps the lower part, were clearly present, those on the outer edges not so definite as those across; just letters recognized as different; just bare letters. This appeared to be almost like a double exposure; first bare perception of the field, and then *Z* and *P* definitely.

E. "In the exposure I recognize nine letters and know what letters they are, then for a fraction of a second there is nothing there, and then the letters which are reproduced are outstanding and clear and the others are not distinct."

E. "I find now that when I recall prominent letters there is a connection between each one as they come to me; seems as if I could draw a line from one to another; they pop out."

These reports seem to indicate that between the specific naming of different items in the field there is, as it were, a sag in experience back to the level of visual pattern, and perhaps still farther back than that.

The manner in which experience will organize is, in general, determined by the instructions. From the instructions in the first series of observations to the instructions in the last series, section 1, there is set for the *O* a trend in the requirement, that is, he is required in the first series to take the field as a whole for the purpose of description, but in the last series he is required to observe a definite letter in a definite location. Here the *O* is not called upon to describe, but merely to name and to reproduce one letter, and to verify or recognize another; whereas in the first set of instructions the *Aufgabe* is description, in the last series it is naming and verifying. The reports coincide closely with the requirements of the *Aufgabe*. In the

[6]E. B. Titchener, *Text-Book of Psychology*, 1910, 542.

last series the field as a whole comes into the reports in relatively fewer cases, but it does appear sufficiently often to enable us to say it is simply because it is ignored that it is not reported, and not because it is not seen as a field.

Summary

We may sum this paper up by stating that, from the reports of our observers, there appear to be three levels in the progress of experience, in visual perception, from its initial moment to its termination. These levels we designate as visual pattern, generic object and specific object; we do not, however, consider them as separate and distinct but as grading the one into the other. The climax of visual perception appears to be the coming forward, or rising up, of certain items in the field exposed, a process which corresponds to the specific naming of the particular items. These items are named consecutively. While the process of specific naming is in progress, the items in the field not being named recede or fade out. When an item has been named it remains momentarily after the rest have disappeared and then recedes or fades out itself. The total perception consists in a gradual but quick rise to prominence of certain items and then a gradual but quick decay.

Conclusions

(1) Our data appear to demonstrate that, in visual perception, there are three main levels, not separate and distinct, but in the nature of a progression, one level developing to another. We designate these levels: visual pattern, generic object and specific object.

(2) Sometimes experience at the level of generic object embodies a number of items as a totality, and this totality, as such, rises to the level of specific object while the units remain at the level of generic object. Thus letters were named as a word unit.

(3) The rise of any item to the level of specific object exhibits itself visually as a coming forward or a rising up from the field in general. With this rise to prominence of one item there is a receding or fading out of other items.

(4) Recognition appears to be the fitting of sense data to existing imagery.

(5) Sense data may be obliterated by imagery which rises to the level of specific object.

(6) Color appears to have an advantage over contour and form in the naming process.

(7) Imaginal overlay may be present in perception.

(8) Secondary reflective factors, almost as immediate as experience itself, may enter into perception.

(9) Visual perception is first a gradual but rapid rise of experience to a climax, which embodies logical meaning, and then a gradual but rapid decay.

AN EXPERIMENTAL STUDY OF MENTAL AND PHYSICAL FUNCTIONS IN THE NORMAL AND HYPNOTIC STATES: ADDITIONAL RESULTS

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A former article[1] dealt with the differential diagnosis of hypnotic from waking consciousness, and brought out that the likenesses in the two states were even more noticeable than the differences in the two states. Specifically, it was held that the normal consciousness and the hypnotic consciousness are alike in showing great variability of performance from person to person and from time to time in the same person; in showing equal abilities in the two states in the fields of sensation, perception, present memory—learning and retention—and physical work not involving fatigue. In hypnosis, however, there is increased ability to endure pain and to recall long-past memories, and decreased ability in some stages, at least, to make continuous responses.

This article will report further experimental results: first, in regard to the capability-differentia of hypnosis from waking; and second, in regard to the nature of hypnosis *qua* hypnosis. For studying the capability-differentia of hypnosis (*i.e.* what one *can do* in hypnosis that he *cannot do* in waking) a strict comparative method is a *sine qua non*.[2] For studying the nature of hypnosis *qua* hypnosis (*i.e.* what the hypnotic *S will do* with conviction that he *will not do* in waking for lack of conviction) the comparative method, although helpful, is not essential.

I. The Problem of Perceptual Acuity: Discrimination of Postage Stamps

Can the normal perceptual powers be greatly or even appreciably augmented in hypnosis? The great majority of writers on hypnotism has held that they can be, *e.g.* Moll[3] and Bramwell[4] report marvelous perceptual acuity in hypnosis. Perhaps the classical case of alleged heightened powers in

[1]This Journal, 36, 1925, 214-232.

[2]The comparative method demands: (1) a large number of *Ss*; (2) a large number of separate tasks; (3) a large number of experimental sessions; (4) many hours of experimentation; (5) tests in both the normal waking and hypnotic states; (6) identical instructions in the tests under the two states; (7) identical conditions in the two states; and (8) control *Ss*.

[3]A. Moll, *Hypnotism*, trans. 4th. ed., 1909, 100 f.

[4]J. M. Bramwell, Hypnotic and post-hypnotic appreciation of time, *Brain*, 23, 1900, 161-238.

hypnosis is that reported by Henri Bergson,[5] in which a boy read out of the cornea of a man in front of him the three-millimeter letters of a book which the man was holding behind the boy's head, the whole image on the cornea being not more than one mm. in height. Other similar results of increased acuity have been almost universally claimed as an outcome of experiments in which the hypnotized *S*, after having been given the suggestion of an hallucination of a picture on one of several identical-looking cards, afterwards picks out the card from the pack.

Three considerations militate against accepting as final this type of experimental result, which is so common in the literature: (a) It is often without scientific control, *i.e.* in measuring the stimulus, standardizing the instructions, or keeping careful time. (b) Even if the experiment is scientifically controlled in the hypnotic state, there has been no bona-fide trial in the waking state of the same *S*s under identical conditions, with the same expectations of accomplishment and demands for putting out genuine effort. (c) The performance of normal *S*s in waking consciousness has been grossly underestimated. Feats of perceptual acuity as marvelous as any in the literature of hypnosis have been performed by *S*s in the normal state.[6]

In order to use the comparative method on this problem of increasing perceptual powers in hypnosis, the following experiment in picking out ordinary postage stamps was devised.

Condition A. At first, the stamps were used just as they appear when pulled from a sheet of a given denomination (say, of two-cent stamps; that is, in an afternoon's experimenting all the stamps were of one kind). After eliminating blurred or lightly-printed stamps, *E* chose one, which he lightly marked with pencil on the back in order to recognize it later himself. The *S* then studied this stamp by itself for 10 seconds. When the time was up, the stamp was taken behind a screen and shuffled with 19 others. Then, when the 20 stamps were uncovered, the *S* would search for the one he had studied. With a few trials, almost any *S* could study a given stamp for only 10 seconds, and, thereupon, either in or out of hypnosis, pick it out from the others which to the uninitiated seem identical. When the *E* saw that with a 10-second study the feat could be accomplished so readily, the time was reduced to a 5-second perusal of the stamp. Five seconds was kept as the time for the later refinements of the technique. Even then, with any sort of observation worthy of the name, the clues from the remains of perforations on the edges were an absolutely infallible

[5] H. Bergson, De la simulation inconsciente dans l'état d'hypnotisme, *Rev. phil.*, 22, 1886, 525-531.

[6] *Cf.* Helen Keller's muscle reading and her sensitivity to other vibrations, in her *The World I live In*, 1914; R. H. Gault's case of synesthesi in a deaf and blind girl, who could distinguish colors by the sense of smell, *J. Abn. Psychol. & Soc. Psychol.*, 17, 1923, 395-401; K. Dunlap's finding that for normal persons the palmesthesic threshold for rate in the neighborhood of 440 d. v. is not more than 36 d. v., and may be as low as 16 d. v., Palmesthesic sensibility, *Amer. J. Physiol.*, 29, 1911, 109 f.

guide. The test was still too easy for ascertaining any alleged difference of ability in the two states; but it is suggested that this feat of itself would pass, if done only in hypnosis, for a good 'stunt' to prove increased powers in that state.

Condition B. To make the task more difficult, the edges of the stamps were cut off, leaving only the portrait surrounded by its own frame; but, even so, the edges could not be cut absolutely square all around: the test was still too easy, and equally easy in the two states.

Condition C. Finally, after other attempts to make the problem difficult enough to ascertain differences of performance in the two states of consciousness, if they exist, a piece of card-board 10x5 in. was taken; 10 round holes ½ inch in diameter and one inch apart were punched in it, and behind these holes the stamps were placed to be inspected, so that they appeared to be, as in fact they were, behind a frame. Only the actual face on the stamps with the meagerest bit of background could be seen. In giving the test, only 5 seconds were allowed for the study of one stamp as it lay on the table looking out through one of the 10 holes. All the other holes were covered. Immediately after the study a shield 12x18 inches was thrust between the *S* and the stamp materials. Then the *E* took the stamp, put it under another hole, and placed nine other exactly similar stamps (*i.e.* any with apparent flaws had been discarded, and, of course, they were all of the same denomination) under the other 9 holes. The screen was then removed and the subject allowed to pick out the stamp he had just studied. The time taken to make a choice was noted. Three trials were made at each session in both states, each time with a different stamp to be studied. Great care was taken to keep the stamps clean; if one became soiled, it was at once discarded. The whole set of 10 stamps was frequently changed. Exactly the same procedure was followed in both the hypnotic and the waking sessions, except as noted in the third paragraph below.

Results. Comparable data under *Condition C* were obtained from 6 *S*s, equally divided among the light, deep, and somnambulistic classes.[7] The results obtained in hypnosis were so nearly identical with those of the same *S* in the waking state that even chance differences seem to have been equalized. When the *E* saw that *one S* after the usual 5-seconds study of the bare features of the face of Washington, could pick out the correct stamp 3 times in 3 trials in both waking and hypnosis, he dropped the test, considering it only too evident that in the past the hypnotic performances were marvelous mainly because the normal waking ability, not having been adequately tested, was considered inconsequential, or else because of the peculiar constitution of the hypnotic subject.[8]

As far as scores go, the first *S* made failures in both states; the second made 3 successes and no failures in both states; the third was successful in 7 out of 8 trials in waking (on account of his high score and the length of time it took to give these tests no comparable data were obtained in the hypnotic state); the

[7] *Cf.* my earlier article for the meaning of the terms.

[8] In the earlier article the fact was emphasized that, in this research, only normal *S*s were used.

fourth was successful in 1 out of 3 trials in each state; the fifth failed in all 3 trials in each state; the sixth *S* failed in 2 out of 3 trials in each state.

The old conception that somehow the formation of an hallucinatory picture on the article to be picked out was responsible for the great success achieved by the hypnotic *S* was shown to be groundless by the results in this case. Given the suggestion of hallucination of a different picture on the stamp he was to study the *S* made on the average a poorer score. For example, if the task demanded that the *S* study a Washington-stamp 5 seconds and pick out that stamp later from ten other Washington-stamps, the *S*'s performance was appreciably lowered in case the *E* led the *S* to believe through hypnotic suggestion that the *S* was really studying a Franklin-stamp which he was later to pick out from among nine Washington-stamps. This fact was ascertained during the testing under *Condition B*, and also in several tests of picking out identical postal cards (as if there were any such things, even though they be U. S. Postal cards!) with and without the hallucination of a picture on the one studied in hypnosis.

Conclusions. This type of experiment is quite different from that which Bergson reports, and in which, no doubt, the boy's eyes were naturally provided with microscopic lenses, but which, probably, he used in waking as well as under the spell of the hypnotist, or possibly used only in hypnosis, because he was led to believe that they could be used microscopically only in that state. It is not denied that some very well-authenticated results show a decided advantage of the hypnotic performance over the waking performance. A case in point is Bramwell's report on the marvelous ability of some serving girls in judging the passing of time.[9] It is contended in this article, however, that in none of these tests showing great discrepancy between the performance in the two states was there as much training given in the waking state or as much consideration given to eliciting a maximum response in that state, as in hypnosis. Since we do not know what the *S*s could have done if the conditions in the waking performance (when there was one at all) had been as favorable as in the hypnotic, it is a gratuitous assumption that the hypnotic state *per se* was responsible for the unusual performance; an assumption all the more unwarranted in the light of the extraordinary performance of normal waking subjects.

Nor is it denied that there may be a type of abnormal person who can do much better in hypnosis than in waking. In individual cases, in which the subject either has not developed his powers to the full or has trouble in giving intelligent direction to

[9]Bramwell, *op. cit.*

his mind, there might be a fairly consistent increase of performance in hypnosis. Final proof, however, that even these cases may show a greatly increased ability in hypnosis awaits the application of a strictly comparable and detailed investigation of their performances in the two states. It can be imagined that certain lack-willed or 'flighty' people may 'find' themselves only in a state of hypnotic trance, in which another directs their consciousness, or in a state of self-induced trance, in which some dominant idea of their own unifies their consciousness.

II. Memory

In addition to the rather extensive tests of memory already reported,[10] there was a series in which two *S*s were tested on memory for articles which they had seen over and over again in the outer of the two rooms used for experimentation but to which their attention had never been called. The tests in both states were given in one session; and were gone through on two different occasions in the inner room.[11] The results were entirely negative. Efforts to 'take' the hypnotized *S* to a certain corner where he said there was something, he knew not what, brought on great trouble in the subject, shown by an expression of discomfort of the mouth and writhing of the body. When pressed to tell what he 'saw,' the subject would say "I don't know," or else, "It's dark, I can't see." Not very often could the *S* be forced to make a wild guess. Whether the fact that he knew that his statement could be checked up, made any difference is questionable, because the same refusal to give details in regard to certain early memories (when the *S* was taken back, say, to his fourth year) was manifest also.

The results of this test, which was given to two somnambulistic *S*s, bear out the results already reported,[12] *i.e.* that there is little if any difference in the performance in the two states in this respect.

Early memory. The tests of reviving early childhood memories were carried on in the following fashion: after record had been made of the results of at least three attempts, in three separate sessions in waking, to find out certain facts of early childhood (*e.g.* with *S* 20, whether as a child he had gone to kindergarten or not, and if so what kindergarten), the *S* was 'taken back' in hypnosis to the time and place in question by being told that he was now a child again. In brief the results were clearly positive in the case of one *S* (two *S*s participating in the experiment), who recovered facts which he could not touch in waking, and of which he could not recognize the truth when told afterward that he had given them in hypnosis, but which were verified in detail by an older sister, who had been

[10]Former article, this Journal, 36, 1925, 223 ff.

[11]For general conditions of these tests the former article or the thesis in the Widener (Harvard) Library must be consulted.

[12]Former article, this Journal, 36, 1925, 223 ff.

with him almost constantly while he was a child. Among such facts were the following:

That he did go to kindergarten in Somerville, that he lived on Dana Street, that Mrs. Chickering lived in the lower apartment; that Mrs. Cole lived next door; that his room looked out upon the back yard and had three windows. This subject in normal consciousness was as much interested in the revived memories as *E* was, because they were as new to him as they were to *E*.

Although, when taken back to early childhood, the other *S* remembered some things that he could not recall in waking, the memories regained were nothing like so much beyond the waking performance as were those of the preceding *S*. However, in the manner of saying the alphabet, of counting, and of saying the name "Connecticut," he fell into childhood habits which he could not reproduce in waking, however hard he tried to recall them. These mannerisms were verified by his mother, who, without being told what the *S* had done in this respect in hypnosis, wrote detailed descriptions of his early habits, writing directly to *E*.

Strata of the personality. The further question of strata of the personality (*i.e.* whether a subject who is 'taken back' to four years of age, is thereupon, in some real fashion, four years old again, and circumscribed by the knowledge that he had at that age) was broached, but by no means settled. Here, again, the *S*s were in the somnambulistic state.

One method was that of asking the subject supposed to be four years old, about such things as he could have known at that age, and then every once in a while thrusting in a question to which he could not have known the answer. This method gave surprising, not to say astounding, consistency in the type of response. *S* 19, for example, in carrying out the hypnotic suggestion that he was four, could count to 13, but could go no farther. Moreover, he counted in a characteristic fashion each time the test was given him: with a certain expression of his face, with an emphasis on certain numbers, and with a way of going back, sometimes several numbers, sometimes all the way to 1. Every time he got to 13 he skipped to 16, 19, 40. Both the subjects in hypnosis would simply say "I don't know" to questions for which they could not have known the answer; *e.g.* when he had been told he was four years of age, *S* 19 knew that his father worked in Bristol but could not by any means be made to say what he did there.

As an objective check on the validity of the four-year consciousness induced by suggestion, the experimenter gave *S* 19 a Stanford-Binet intelligence test. The behavior during the various parts of the test was all that could have been desired of a four-year old child; cocksureness, hesitation, curiosity, even humor of a quiet sort (in regard to the mutilated pictures and in telling the differences between an egg and a stone) were shown. The results, however, showed an I. Q. of 183 if his age be taken at 48 months (*i.e.* just 4 years of age). If, however, as is more reasonable, he be accounted at least 57 months old (4 years and nine months) the I. Q. is 154. The age of 57 months is used for the following reason: earlier in the evening, after he had been told he was four years old, the *S* said he had just been scolded for being out in the snow. Since his birthday is late in March, and since in hypnosis he never recalled any birthdays, it is likely that this evening he was, in hypnosis, back in the winter following his fourth birthday. This *S* is a very good student; but whether an I. Q. of 154 represents what he could have done at four years of age can not be determined now. This *S* was not put through a similar test in waking, with the instructions to act as a four-year old child would, to see if he could by conscious simulation approximate his score in hypnosis; but two other *S*s (one an equally good student, and the other only of fair standing) did play this game of being four years old. By counting each of their ages

also as 57 months, the first showed an I. Q. of 144, the second an I. Q. of 189. Neither of these *S*s was previously acquainted with the Binet scale.

Judging from the above results, the idea of definite strata of the personality is left without conclusive proof. To have come to a definite opinion one way or the other would have taken more time than *E* had to spare to the question.

A statement of the *S* to whom the Binet test was given in hypnosis, made after being awakened, that he had just been dreaming that he was in a tomb in Egypt, speaking to a wonderful woman, telling her in grandiloquent language about himself as a child, makes the *E* believe that, at least in this case, the *S*, all unconsciously and with great verisimilitude, was playing a rôle which was in some fashion witnessed by another part of the personality than that which was directing the acting. It may be said that within the limits of the four-year old suggestion, there was great freedom of action. But that the *S* is actually circumscribed within the limit of the early memories, or that the dissociation of personality is so complete as to leave a four-year old personality intact, is hard to believe, if for no other reason than that the very voice to which he is responding has no part in those early memories, and yet somehow in hypnosis is indissolubly bound up with them.

This series of experiments seems to point to an actually increased power of remembering past events which is in striking contrast with the merely equal ability in remembering present matters. This conclusion is the same as that of most men who have done experimenting in this field, notably Binet and Feré[13] and Loewenfeld.[14] The fact that some of the memories are not true in minor (or important) details, does not invalidate them as memories, any more than one's mistake in the year in which he received a certain injury invalidates his memory of having received the scar.

III. Post-Hypnotic Amensia

On no question in regard to hypnosis has there been greater disagreement than on that of post-hypnotic amnesia. One group of writers, including Bramwell and Wingfield, considers it as the distinguishing mark of the somnambulistic state; another group, including Bernheim and Loewenfeld,[15] thinks that it may or may not follow upon somnambulism. Bernheim held that the memory for the hypnotic events could always be recovered.

In order to deal summarily with what could be dealt with at great length, *E* will only state that his experimentation confirmed in great part the opinion of Loewenfeld and Bernheim, give the experimental results, and draw certain conclusions from them.

As a measurable method of judging the amount of amnesia which hypnosis carries with it, the adjective-noun, logical association, etc. material of experiments 10, 11, 12, and 13 of the earlier article was used.[16] The procedure was devised in order to give three sets of data: (a) the amount of amnesia which spontaneously occurs from hypnosis; (b) the

[13] *Cf.* J. Grasset, *Hypnotisme et suggestion*, 1916, 283.
[14] Moll, *op. cit.*, 145 f.
[15] L. Loewenfeld, *Hypnotismus und Medizin*, 1922, 141.
[16] See former article, 224 f.

amount when the *S* in hypnosis is ordered to remember; (c) the amount that occurs when the *S* is ordered to forget. Scores were obtained from the light, the deep, and the somnambulistic *Ss*; but since the somnambulistic were the only ones who differed noticeably in the results under the three types of instructions, only their scores are of interest here.

It is to be kept in mind that to request the just awakened *S* to tell all that he remembers of the seance just ended, or to ask him if he remembers this or that, is an entirely different matter from making measurable tests with materials which have been definitely learned and scored in hypnosis and which can also be definitely scored in the succeeding waking period, both by noting what is retained and what is saved by relearning. Furthermore, the validity of the tests was assured by the *S* not knowing how, when, or whether he had learned these materials.

Results. *S* 19 on awaking from hypnosis sometimes could give only the meagerest report of the hypnotic session, and sometimes could give none at all. This *S* without any suggestion as to remembering, made practically perfect scores in the post-hypnotic quiz of what he had just learned in hypnosis; made the same high scores, of course, when he was ordered to remember; but when he had been ordered to forget made scores of about 80% (and that only with the greatest of mental effort, accompanied by doubt and hesitation). *S* 22 when asked on awaking what he remembered of an hour and a half of tests given in hypnosis, would, characteristically, say that he seemed to have been talking to somebody as he lay on the couch for *two or three minutes*. Even without a suggestion, *S* 22 retained all the associations learned, but did not remember *when* he had learned them. When, however, he had been given the suggestion to forget, this *S* was, for a time, hopelessly at sea in the immediately succeeding session; but, even so, on the average gave 50% of the associations correctly. In this case he would seem to remember that he had learned the list at a session the week before—in a waking session, so he said. *S* 21, after an hypnotic session, could report sometimes on one or two incidents in the hypnotic sleep, *e.g.* on having seen "a bright card" on which he had really had the hallucination of a certain picture; or sometimes on none at all. This *S* made practically perfect scores with and without the suggestion to remember; although he always affirmed that he had no memory of having learned the associations—"they just come." Under the suggestion to forget, however, all the associations were lost, in the immediately succeeding waking session, but not altogether for later sessions of waking. *S* 20 on waking always said merely that he had dreamt he was here or there, and that he could give no account of anything that had happened, although such a statement is not strictly true, because twice out of thirteen seances he remembered 'seeing' himself doing one of the tasks. *S* 20 not only disclaimed all knowledge, on awaking, of what he had just learned in hypnosis, but also failed in hypnosis to recognize what he had just learned in waking. And yet this *S* did not show complete amnesia when given definite tests. For the first two sessions he, like *S* 21, gave merely haphazard associations, but later, when a great number of associations—some of which were learned in hypnosis and some in waking—were in his mind, the difference between what had been learned in the two states became progressively less. With this *S* no suggestion at all was as powerful as the suggestion to forget. When, however, he had been ordered to remember, he retained the associations, but not the feeling of having learned them. It seemed to him as though he had stood outside of himself and had watched himself learn them.

As one notices, the above results were results for an hypnotic session which immediately followed a waking session, There were, for each of the *Ss* referred to, an average of 3.5 of these double sessions. The amount of the post-hypnotic amnesia, however, for all *Ss* became progressively less

with time and with the number of associations which were, so to say, jostling for emergence, so that after a few weeks (or even a few days, if several sessions were held within that time) the distinction between what had been learned in hypnosis and what had been learned in waking had almost entirely broken down.

By another refinement the effect of suggestion was made manifest. By telling *S* that, when he awakened, he would be unable to recall any of the associations until a certain signal had been given, the fact of very great differences in the ability to remember before and after the signal, were established. The result, as one would suppose, varied with the *S*; but they were noticeably different with every somnambulistic *S*.

Conclusions. (a) That post-hypnotic amnesia varies greatly with the *S* is a conclusion in keeping with the contentions of many of the writers on hypnosis.[17] (b) It is exceedingly doubtful that there is ever total post-hypnotic amnesia. The *S* may not, of course, be able voluntarily to recall the events of the seance; he may not be able even to recognize them when they are rehearsed to him; but that he has retained something of them seems clear, at least in this experiment, from his giving in waking associations learned in hypnosis, and from his bettered score in relearning in waking what had been taught him in hypnosis. (c) Consequently, in reporting a *S* as exhibiting post-hypnotic amnesia, the *E* should always specify whether the *S* cannot recall. or cannot recognize, or to what extent his retention of the hypnotic activities has been affected as shown by his post-hypnotic performance. (d) The more numerous and the more similar the materials learned in the two states, and the longer the time since the learning, the less likelihood of the *S* keeping the hypnotic memories dissociated from the waking memories. (e) The Freudian theory of forgetting seems to be borne out in the effect *E*'s suggestion to forget has in lowering the post-hypnotic performance. (f) Post-hypnotic amnesia is no more valid a criterion for genuine hypnosis than dreamlessness is of real sleep. The fact of remembering does not militate against the reality of one or the other.

IV. Induction of 'Color Blindness'

The materials for this test were the following numbers of the Holmgren set: 27, 32, 34, 36, 39, 43, 45, 48, 58, 60, 61, 62, 63, 71, 75, 76, 82, 84, 85, 87, 88, 90, 92, 93, 94, 97, 98, 100, 108, 111, 113, 117, 118, 121, 126, 129, 131, 132, 134, 136.

After *E* had given the suggestion to the hypnotized *S*s that all the *red* and *green* skeins in this collection were now yellows, and that they differed only in brightness, he asked the *S*s to pick out all the yellows and to put them in a series, with the brightest yellow at the extreme left and the darkest yellow at the extreme right. Before the task was undertaken, *E* saw to it

[17] *E.g.* J. Delboeuf; see Moll, *op. cit.*, 58.

that the suggestion had been accepted, by having the *S* pick out various 'yellows.' In the waking session, which in this experiment always occurred after the hypnotic, the same yarns were used; but this time with the instructions to pick out the *yellows*, *greens* and *reds*, and to place them in a series according to their brightness values, without any regard to their colors.

Results. *S* 19 made the following arrangement in waking; the first number representing the brightest yarn, the last representing what he considered the darkest: 76, 61, 62, 43, 117, 88, 36, 58, 131, 94, 118, 132, 85, 46, 45, 126, 98, 121, 136, 129, 90, 60. The following is his selection made in hypnosis: 76, 93, 61, 118, 90, 88, 94, 43, 45, 132, 129, 126, 36, 98, 27, 58, 62, 131, 85, 121, 60. On awaking from hypnosis, he said he remembered putting some yellow yarns in the order of their brightness.

S 20 made two selections in waking and two in hypnosis; only one each will be given. One arrangement made in *waking* was as follows: 62, 94, 61, 88, 58, 36, 76, 118, 93, 43, 117, 85, 46, 45, 126, 129, 132, 121, 60, 111. The following numbers show one selection made in hypnosis: 76, 93, 61, 62, 43, 85, 117, 88, 58, 36, 118, 94, 131, 27, 98, 121, 136, 45, 126, 132, 129, 133, 108, 90, 134. After awaking, even when shown the yarns, this subject knew nothing of what he had done.

S 21 gave the following order in *waking*; noting only one of three trials, all very much alike in results: 46, 136, 60, 131, 85, 121, 98, 58, 36, 62, 27, 132, 117, 43, 129, 61, 94, 93, 76, 88, 118, 45, 126, 90; and the following in hypnosis, 46, 136, 131, 121, 60, 58, 36, 132, 85, 94, 62, 98, 117, 88, 61, 43, 27, 129, 118, 93, 45, 76, 126, 90. On being awakened the *S* remembered having seen some greenish-yellow and reddish-yellow skeins.

S 22's choices are typified by the following order of numbers: in *waking*, 90, 129, 133, 88, 126, 45, 132, 93, 76, 118, 61, 62, 117, 43, 85, 36, 58, 94, 27, 108, 131, 86, 121, 136, 60, 46; in *hypnosis*, 90, 111, 129, 132, 45, 126, 133, 93, 76, 61, 108, 118, 94, 27, 98, 121, 58, 36, 88, 131, 62.

Whether these results are anything more than a curiosity is very questionable. They are given in order that those interested in color theories may interpret them to their heart's content. Whether the subject did or did not see the reds and greens as yellows, is a problem which strikes at the very nature of the hypnotic consciousness, but into which, for lack of a refined technique, *E* could not go.

V. Spontaneous Catalepsy

E's results agree with those of Moll, who concludes his discussion of the question as follows: "I have hardly ever clearly seen a typical *flexibilitas cerea* in hypnosis, except when the training of the subject had been directed to that point."[18] If *E* begins with passes, then proceeds to bend the *S*'s arm, keeping the elbow on the couch and holding the hand in the air ever so short a time, *S* gets the suggestion that the thing to do is to leave the hand where it has been placed. From this simple posture, to one in which the leg, when raised, keeps the position without falling back to the couch, is only a matter of further, unexpressed, but still very easily felt, suggestion. By avoiding manipulating the limbs of the *S*s, the present *E* found that none

[18]Moll, *op. cit.*, 80.

of the somnambulistic *S*s ever showed any tendency to spontaneous catalepsy. In the very deepest stages, an arm when raised fell back to the couch with a thud. A greatly increased susceptibility to plasticity of the limbs does occur, but the popular conception that the arm or leg will mechanically stay in any position it is placed, without suggestion, seems to have arisen from the method of inducing hypnotism by stroking and manipulating the limbs. Really, the limb seems to stay in a certain position because it was *put* there. If there is never any moulding of the *S*'s limbs, an arm when raised, just as innatural sleep, falls like so much lead.

VI. Rapport

The phenomenon of isolated rapport, *i.e.* the state in which the hypnotized *S* attends only to the experimenter, was, for the most part, verified in the case of somnambulistic *S*s, but not for any others.

VII. Future Problems

Some problems, aside from the question as to the difference in capacity in the two states, which should be investigated are the following:

(1) The question of strata of the personality.
(2) The effect of waking purpose on hypnotic performance.
(3) The amount of initiative compatible with hypnotic sleep.
(4) The actuality of hypnotic hallucinations.[19]
(5) Humor in hypnosis.
(6) Artificially induced color-blindness.
(7) Whether a person is susceptible to more training, *e.g.* in discrimination, in hypnosis than in waking.

VIII. General Conclusions

In addition to the observations at the end of the previous article,[20] *E* would stress the following points.

(1) The results described in this article, as far as they affect both waking and hypnosis, seem to confirm conclusions drawn in the earlier article; namely, that on the whole there is no noticeable difference between the normal and hypnotic states in the ability of normal persons in the fields of sensation, perception, fine discrimination, present memory (learning and retention), or physical work which does not involve fatigue. This research, however, has shown, as many experiments heretofore had proved, that (at least in some persons) memory for long past events is much better in hypnosis than in waking.

[19]See B. Sidis, Are there hypnotic hallucinations?, *Psychol. Rev.*, 13, 1906, 239.

[20]Former article, 230 ff.

(2) The results, as far as they affect only hypnosis, agree with the prevalent conception that rapport spontaneously appears in the deeper stages of hypnosis. They agree also with the idea that memory is greatly affected in the post-hypnotic period. The results, however, are distinctly negative in regard to spontaneous catalepsy and complete post-hypnotic amnesia as differentiae of any grade of hypnosis. Definite proof, in measurable form, has been found for widely different degrees of post-hypnotic amnesia, even in somnambulistic *S*s. The suggestion is offered that hereafter an *E* who reports a *S* as amnesic for the hypnotic incidents, should specify what form and what degree this post-hypnotic amnesia takes.

(3) The hypnosis met with in these experiments seems to be self-hypnosis. *S* 19 in normal life exhibits powers of concentration bordering on deep abstraction; *S* 20 often goes into a reverie amounting sometimes to an ecstasy, while, as priest, he is counting his beads; *S*s 13 and 14 can put themselves into natural sleep almost at will; *S* 21 has several times hypnotized himself; *S* 22 invariably goes to sleep while riding home on the train, and wakes up just when the train gets to the stop before his own. All that *E* added to these men was a better chance to concentrate than usual. It seems that prestige is an effective means of bringing about a concentration of the attention, *i.e.* if an important person is saying something, it must be worth listening to.

(4) For the most part, hypnosis seems to consist in taking, with great conviction, an attitude of mind. Even though all mental functions in the two states should be proved to be equal, still the differences in what the subject will and will not do, *i.e.* his *ability* to take an attitude, are enough to differentiate the two states. This ability to play a part, so to speak, and yet be quite unconscious that he is playing it, may effect increased memory for long past events in much the same way that, with some persons, throwing of the body into a certain posture may enable them to recall incidents in connection with a certain man who habitually exhibited that posture. This taking of an attitude (consequently *refusing* to feel the pain or even to take cognizance of it) seems to be a description of the state of mind when analgesia or resistance to fatigue is induced in the hypnotic subject.

The basic difference, then, between the normal and the hypnotic states seems to be a difference, not in strictly mental or even physical abilities, but in the attitudes which can be assumed with verisimilitude. These hypnotic attitudes are emotional, impulsive, and voluntary rather than intellectual. "This would leave the term 'hypnosis' with a fairly definite meaning: a state in which a person *will do*, in a *bona fide* manner, possessed of conviction, what he *will not do* in waking life for lack of such conviction."[21]

[21]Former article, 232.

A METHOD FOR THE EXPERIMENTAL PRODUCTION OF EMOTIONS

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A description is here given of a method for the production of emotions in trained and sufficiently willing *O*s which was successfully employed during a series of experiments on the blunting of emotions.

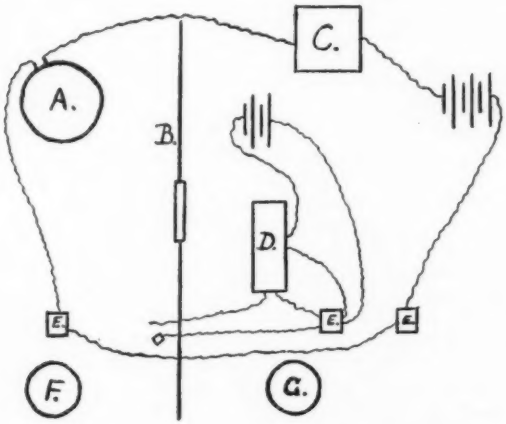
Colours (or nonsense syllables) are presented in rapid succession and in different series and combinations to an *O* who has to perform given reaction-tasks in connection with them. The *O*'s success or failure in the performance of a particular task is (unknown to the *O*) determined by *E* who causes this to take place under different conditions and in various cases to be described later. The success or failure under a given condition becomes the causal antecedent for a particular emotion to arise.

The colours (or nonsense syllables) are presented to the *O*s by means of a Ranschburg memory-apparatus with two reaction-keys, one for *E* and one for *O*, inserted between it and the cells. The discs, which are placed in the memory apparatus and whose divisions are coloured according to a definite scheme, are caused to revolve, a section at a time every $\frac{1}{2}$-sec. to $\frac{3}{4}$-sec., *e.g.* by means of a Bowditch-Balzar contact-clock as supplied by Zimmermann. Punishment can be applied when necessary by means of shocks from a small inductorium. *O* is separated from *E* by means of a partition and the apparatus should be placed as far as possible on *E*'s side.

The arrangement of colours on each disc differs from that on the others. The first type of experiment is carried out with a disc on which the colours are so arranged that *O* can fulfil his instructions. The divisions are *e.g.* coloured as follows.[1] Two of them, separated by a more or less equal number of divisions, are pale green each being spotted with three black dots. With the view to forming easily avoidable traps other divisions at irregular intervals are coloured somewhat similarly; *viz.*, brown with three black dots, pale green with a dash and two black dots, pale green with "3 dots" written on it, white with three green dots, black with three green dots, pale green with the figure 3 succeeded by three black dots and another 3, dark green with three black dots, pale green with a dash, three black dots and

[1]Only a horizontal strip across the middle of each division should be coloured, in order to prevent the *O*s from seeing the following colour prematurely.

another dash, yellow with three black dots, and pale green with three dashes. The remaining divisions of the disc are painted arbitrarily. Before each experiment *O* is instructed to react by pressing the button of his reaction-key as soon as pale green spotted with three black dots appears.[2] He is warned that he will be punished for an incorrect reaction. As soon as *O* reacts *E* announces his judgment of the reaction as 'right' or 'wrong.' The object aimed at with the experiment with this disc is to cause *O* to react correctly under conditions which create in him



the impression that his correct reaction is a praiseworthy achievement. This impression is created by means of possible inducements for faulty reactions which are introduced into the arrangement of colours; these 'traps,' given above, are however so constructed that *O* can readily recognise them as such and avoid them without being aware that this avoidance is what is expected. Introspection reveals that satisfaction normally accompanies the correct reactions although blunting sets in after a few repetitions.

The following are examples of reports obtained in connection with experiments carried out as described above.[3]

[2]The *O* is told to hold down his key. This enables *E* to break the circuit and so prevent a further movement of the disc since this will divert the attention of the *O* and since it is desirable that *O* shall see the result of his reaction plainly before him.

[3]These reports have been translated as accurately as possible from Afrikaans in which language they were given.

Satisfaction. (1) A.[4] I was prepared to react when I saw pale green with three dots. I think that this was so because my attention was well concentrated on the task and nothing distracted it. I reacted as soon as the colour appeared and with a fair amount of satisfaction realised that I was right. A slight uncertainty prevailed for just a moment but I was soon convinced that my reaction could not be wrong. *E*'s judgment increased my satisfaction and also pleased me as I now knew that the unpleasant punishment could not be applied this time.

(2) B. My attention was riveted on the rotating disc—more so than on the task to press the button of the reaction-key. I thought (I do not know exactly when) that the latter action depends upon perception of the former. A very high state of tension was present during the rotation of the disc. It was unpleasant and contained, it seems to me, something of fear or anxious expectation. While the disc was revolving I was chiefly aware of the fact that wrong reactions and punishment were possible, as well as of a desire to evade the latter. I do not, however, remember any clear desire for correctness of reaction as such. The relief which arose when I saw that I was reacting correctly was not very strong; I undoubtedly plainly felt satisfaction when I became conscious of the disc standing at the correct combination of colours. This emotion remained for some time. I feel sure that it did not disappear directly since my attention was distracted by a sudden movement of the disc[5] and yet I immediately afterwards noted that the consciousness of success accompanied by satisfaction was still present. The satisfaction did not follow my reaction at once but only arose when I realised that I had been successful, *i.e.* when it was as if I praised myself. I do not remember any thoughts relating to the *E* in connection with my satisfaction. The satisfaction was practically the same kind of emotion (although not so intense) which one feels when one has just completed an important piece of work well.

(3) F. I watched the disc fixedly and wondered whether I would react correctly or not. Several apparent reaction-colours passed; but I did not feel much tempted to react. The disc seemed to move rather rapidly. I reacted correctly and knew that this was so. I had a pleasant feeling of satisfaction—no delight—for although owing to former correct reactions I did not look upon my reaction as something exceptional, yet it was nice to react quickly and surely. The satisfaction was of the same kind that one feels when one has made a good stroke in a fast set of tennis. I do not remember thinking about the punishment at all.

(4) G. My attention was firmly fixed on the disc as I awaited the appearance of the reaction-colour. I reacted with decision when it appeared and when I saw the disc stand at the correct position a strong satisfaction with my action made itself felt. I thought that the *E* could not do otherwise than judge my reaction favourably. I fully realised that I had completely fulfilled his instructions. The idea that I was free from any possibility of punishment was only vaguely present when the *E* said 'right' and the satisfaction increased.

(5) H. I desired to react correctly since it is unpleasant to be unable to fulfil one's task, and made a special effort to do so. My attention became well fixed on the disc towards the end of the experiment when I expected the reaction-position. I remembered the punishment and thought that by reacting correctly I would prevent the *E* from applying it. Then I saw pale green with three dots and hurriedly pressed my button. I was so glad that my reaction was correct that I nearly laughed. I was well

[4] The letter preceding each example serves to denote the *O* whose report follows in case comparison with other reports by the same *O* in connection with the further types of experiment should be desired. The letters also indicate how the reports cited were given by various *O*s.

[5] *E*'s carelessness.

satisfied for I knew that my reaction was a good one and the fulfilment of my wish. I also remembered that I would now receive no punishment and thought that I deserved to escape it. All this made me feel pleased and satisfied. Even now I feel quite cheerful.

The colours on the disc for a second type of experiment are so arranged that the *O*s will be induced to react at a position of the disc which is not the correct reaction-position. Seven divisions of this disc are each coloured in three equal sections of blue, red and green, respectively, and an eighth division blue, vermilion and green. These eight divisions are separated from one another by irregular numbers of spaces. Each of these eight combinations is immediately preceded by a blue division which in turn follows upon a yellow one.[6] The remaining divisions of the disc are painted other colours, with the exception of a few cases of yellow, not followed by blue, which serve to keep the *O*'s attention fixed on the disc. The *O* is instructed to react whenever the blue-red-green combination appears, and is told that this takes place after the appearance of a blue division preceded by a yellow. He is asked to endeavour to react correctly in every instance and is warned that a reaction when blue-vermilion-green appears is quite wrong and will be followed by punishment. The difference in the discs is shown him beforehand. *O*s are usually entrapped the first time if the disc is so placed in the memory apparatus that they must react at the seven correct reaction-positions before the wrong combination appears. The conditions can however be kept favourable for entrapping the *O*s in repetitions of the experiment with this disc. *E* attains this end by varying the position of the disc after each appearance of the trap so that *O* never knows how often the true combination will be exposed before the trap again appears. Now and then, for instance, by adjusting the disc before each experiment, the trap is allowed to appear several times in succession. The rapidity of the motion of the disc must also be adapted to the *O*s in order to obtain the best results but although the disc should move rapidly enough to entrap the *O*s the movement may not be so rapid as to cause the task to appear impossible to them. The *O*s are punished with a painful shock whenever they are entrapped. Usually, before blunting takes place, keen disappointment, and sometimes regret, is felt in connection with in-

[6]The combinations are not allowed to appear without warning-colours although a sudden appearance of the trap will apparently favour incorrect reaction. The absence of warning-colours is however disadvantageous in as far as undesired mistakes such as late or no reactions will then occur more easily at the correct combinations. Besides this the presence of warning-colours has the advantage of causing the *O*s to be sufficiently prepared for reaction since these colours are for the *O*s par excellence the warning-colours in connection with the true combinations even though they also precede the trap.

correct reactions at the trap, and exaltation or elation in the few cases when an *O* is not caught in the trap. Satisfaction is felt at the usual correct reactions at the true reaction-positions and is especially pronounced after the *O* has been entrapped several times in succession. Even delight is sometimes felt when the *O* is brought under the impression that he is reacting correctly at the ordinary reaction-positions under specially difficult circumstances, *e.g.* after the *E* has altered the position of the disc. Shame is sometimes felt in connection with this type of disc when an *O* is caught in the trap and when the realization of punishment *for a silly mistake* predominates over the realization of an incorrect reaction. A variation from the ordinary routine of the experiment can be introduced by informing the *O* that he will receive no punishment for incorrect reactions at the trap since such failures are in themselves sufficient punishment. According to the *O*s' reports the disappointment is very often just as keen as in the other cases.

Disappointment. (1) A. I remember that at the beginning of the experiment I wished to avoid an incorrect reaction. I was interested in the experiment and attempted to react correctly. I sat looking attentively at the disc in a state of readiness for reaction. The warning-colours made me still more attentive. While I was pressing the button of my reaction-key, or immediately afterwards, I remember looking at the disc to make sure whether I was right or wrong. I was really very much disappointed when I saw that I had been caught in the trap, which I had really wished to avoid. The disappointment arose when I became aware of the fact that I had reacted on the blue-vermilion-green and that this was wrong. The disappointment was afterwards accompanied by a tendency to an embarrassed kind of laughter.

(2) C. This time I again began with a fair amount of self-confidence, although I was quite nervous. I felt that I had a certain reputation to maintain and was afraid that I should lose it. I intended to look out for the yellow as I wished to make sure that I would see the yellow strip. A certain colour appeared which confused me as it resembled yellow. Expectation of the warning-color blue immediately filled my mind which then wandered for I began to think of the effect of the blue colour on me. I felt very uncomfortable when I noticed my distraction. Just then I saw blue disappear in the slit. As the combination appeared I thought for a moment that it was the wrong one, but I felt as if I could not decide soon enough with any certainty and reacted. I had only just reacted when I saw that I had made a mistake. My disappointment was very great especially when I realised that I should have acted according to my first impression. This disappointment became still more accentuated when the *E* said, "Trap—wrong." The punishment served to make me realise more emphatically that I had done wrong.

(3) D. I was in a very high state of tension and did not remember the warning-colours properly. I did not think of the wrong combination at all, but was quite determined to react correctly. I forgot about the yellow altogether and no colour had any effect upon me until I suddenly saw blue. Its appearance startled me and somewhat confused me; but was nevertheless followed by an expectation of the right combination. I also noticed the blue and green portions when the combination appeared but felt quite convinced that it was the correct one. I reacted with decision and a fair

amount of satisfaction. My disappointment was altogether painful when the *E* said "Trap" unexpectedly and when I saw that I had indeed been entrapped. This disappointment was accompanied by a little shame at my having been so sure of the correctness of my reaction.

(4) E. I recognised blue as warning-colour and sat ready to react at the following position. I was partially in doubt what to do when the following combination appeared and reacted more or less automatically. Then I saw that I was wrong. An emotion of disappointment and dissatisfaction with myself arose and I thought that punishment would undoubtedly follow. The punishment was painful and after its application disappointment with my incorrect reaction increased. My whole condition was very unpleasant; I do not know whether the painful sensation or the emotion was the chief constituent—at any rate both were pronounced and in the foreground of consciousness.

Regret. (1) F. I sat awaiting the reaction-position very determined to look well and react correctly. Once or twice yellow drew my attention but was not followed by blue and the combination. When yellow again appeared after these 'bluffs' I expected the correct colours to follow. The fact that there was a combination of colours finally attracted my notice more than the colours themselves. Owing to this I reacted. I immediately saw that I had reacted incorrectly and the disappointment and regret were so strong that I even heaved a sigh involuntarily as I released the button of the reaction-key.

(2) G. I was in a high state of tension especially after a few colours had passed without those appearing on which I had to react. I again determined to react and was quite ready for reaction and every time slightly pressed the button. When the wrong combination appeared the expectation was so strong that I reacted unintentionally. Just as I reacted I saw that I was doing wrong and tried to jerk back my hand in time but it was too late. I thought that I should have escaped this mistake altogether and that I had only myself to blame for it. I felt a fairly pronounced regret.

Exaltation. (1) C. I began this experiment in a higher state of tension than on previous occasions, since I expected the wrong combination but was afraid that I would not recognise it. I did not wish to react at the wrong combination. I was very plainly conscious of the punishment-factor in the experiment and was afraid of it. The consciousness of possible punishment made me nervous and this nervousness increased while the *E* delayed the movement of the disc. I became calm and collected when the disc began to move and watched the colours with a fair amount of self-confidence. When yellow appeared I was immediately on my guard and my mind was completely filled with thoughts in connection with the wrong combination. Expectation of the wrong combination increased when I saw the blue strip. When the combination itself appeared I became confused and nervous for a moment. Then I recognised the combination as the wrong one and with extreme self-satisfaction and a decided feeling of elation or exaltation I sat still looking at it. Together with the elation was the thought that I had not been entrapped this time. I felt disappointed when the *E* did not say "right" as is usually the case and as I expected. I expected praise and commendation most decidedly and in fact also desired it.

(2) E. After yellow and blue the combination appeared. Although quite prepared for reaction, something hindered me and seemed to prevent me from reacting. (I do not know what it was.) Then I recognised the vermilion and although the doubt as to whether I should react or not, which had been present at first, now disappeared, something of fear persisted until I realised with perfect certainty that I had done right to allow the trap to pass. Then I felt quite a strong satisfaction and a real feeling

of exaltation which made me generally pleased. When *E* stopped the disc I again doubted for a moment whether I would be punished or not but this doubt was very transitory.

(3) G. In connection with the combination blue-vermilion-green there was a very strong feeling of satisfaction with the fact that I had not been entrapped this time. An inclination to laughter and a feeling of triumph were also present. These emotions arose in connection with the thought that *E* did not succeed in catching me in the trap. At the commencement of the experiment I became aware of a desire not to react incorrectly. I was aware of strong interest in the experiment and in my task, and I concentrated my attention on the colours to be ready for reaction. The situation was a pleasant one. I cannot remember that thoughts in connection with the trap were present during the rotation of the disc. Heightened attention followed the recognition of the warning-colours and it seemed to me as if my fingers contracted slightly when blue-vermilion-green appeared. The satisfaction and elation did not last long and I sat wondering what would happen next.

Satisfaction. C. I felt hopeful although I had reacted wrongly on the former two occasions. I thought that I ought to remember the succession of the warning-colours and combination properly, and decided to concentrate on recognising the yellow strip. I decided to react correctly and was in a high state of tension. When yellow appeared I expected blue. I felt somewhat relieved when blue actually did follow and thought that I was expecting the colours in the correct sequence. The combination then appeared and I recognised it immediately. I reacted correctly when I saw the ordinary true combination. There was a very strong satisfaction when I reacted correctly and although I was sure of my reaction it became stronger still when *E* confirmed my judgment. I then felt that I had been able to depend upon myself to react correctly and in connection with this there was a slight emotion of pride which increased somewhat when I thought that *E* must be satisfied with me.

Delight. F. The tension was very great this time for owing to the alteration of the position of the disc by the *E* I did not know when to expect the reaction-colours.[7] They might even appear immediately. I was surprised when several colours passed by without the warning-colours appearing for I thought that the *E* had placed them very near the slit. I nearly reacted at times simply owing to the tendency to press which I could feel in my fingers. When the combination appeared I felt uncertain and hesitated and so nearly reacted too late. The delight was very strong indeed when I saw that I was right and when the *E* said so too. I was well aware of the fact that I had nearly made a mistake and also that the *E* by moving the disc had increased the difficulties so that my reaction under the circumstances was really an achievement.

In a third type of experiment no reaction is required of the *O*s; but they are instructed to pay careful attention to the disc which is allowed to revolve comparatively slowly. The colours on the disc are arranged as follows; yellow, red and purple succeed one another in this order at seven places; at intervals yellow, or yellow succeeded by red, is inserted in order to secure the *O*'s constant attention on the disc; and the remaining divisions are painted other colours. The *O*s are told that yellow, red and purple will succeed one another at intervals and that a severe

[7] The increased difficulty was only imaginary for in reality the *O*s never knew with any certainty when to expect these colours.

shock (of the painfulness of which the *O*s are given a foretaste) will be applied whenever purple appears in the slit of the memory apparatus. Expectation of the shock is produced by the colour in question being preceded by warning-colours which the *O*s know as such. The appearance of the warning-colours makes the conditions favourable for fear to arise and this actually normally took place during experimentation. The *E* stops the disc on purple before applying the shock. Relief can be produced by a similar experiment if a mild shock is given after several severe ones—the *O* must however then be asked for introspection a few seconds after the shock in order that the relief may first assert itself plainly.

Fear. (1) A. I sat looking at the colours with a certain amount of tension and had the expectation that a painful shock would be applied. Nevertheless I smiled although this smile contained something of fear. It was a pained kind of tendency to laughter and embraced an unpleasant feeling of impotence. I also wished that the colours would appear quickly so that the shock could be given and become something of the past. Suspense was present throughout the experiment. I intended to bear the pain with resignation and not to allow it to upset me. When yellow appeared I thought that red would follow but once or twice it did not. At last red appeared after yellow and immediately there was a heightened fearful expectation of the shock. I disliked thinking of the latter. Then purple appeared and the thought that now punishment would follow directly came clearly to my mind. It was as if I prepared myself for something very unpleasant and the fear increased until the shock was actually applied.

(2) B. I again thought of the task. When the disc began to revolve I immediately experienced an emotion of fear in connection with the expectation of what was about to happen. This feeling increased especially when yellow and then red appeared for the pain seemed to come nearer. Then I saw the purple in the slit and I attempted to resign myself to the inevitable, *i.e.* I tried to suggest to myself that I would not feel the pain. But this thought did not help at all, and the pain was rather more intense than before.

(3) G. I became slightly afraid as soon as the disc commenced its movement. I remember that I thought that my feeling must in a slight degree resemble that of people about to be shot or hanged who hear the footsteps of their executors and cannot help knowing that their end is near. When the warning-colours appeared I knew similarly that I would receive a painful shock which I dislike very much. There was also a certain amount of tension. When yellow appeared I realised that the shock would soon follow and the former slight fear became stronger. I noticed that I was afraid of the pain and desired that I should not be given the shock. This fear became more intense still when purple appeared so that an unmistakable emotion of fear was present when the shock was at last applied.

(4) H. I paid careful attention to the disc and firmly expected that *E* was going to apply the shock at the first occurrence of purple. I was in a state of continual expectation of the shock and attempted to distinguish the colours properly so that I would know when it was due. I was very much afraid. I saw yellow and then red and then did not feel that it was necessary to look out for purple since I knew that the shock was coming. I felt more afraid while these colours were appearing than I did before since it seemed to me that they followed each other too soon to enable me to become mentally prepared for the pain. *E* then punished me and I soon wished that he would stop. I called out: "It hurts!" and hoped that my

exclamation would move *E*. It seems to me that the fact that the punishment which is very painful was repeated during this period a number of times in succession accentuated my fear for no respite was given. Yellow which used to be a colour I liked is becoming quite distasteful to me, and purple has been very disagreeable all along.

Fear and Relief. A. In a few cases yellow appeared and I remember thinking that I would receive a shock. I clearly noticed an increase of the fear and dread. In one case yellow was followed by red and I firmly expected purple and punishment. The fear and dread were quite intense while red was exposed and a well-defined relief arose when the succeeding colour was not purple. This relief seemed to be chiefly connected with my breathing. Then my attention again became fixed on the colours and in my mind the idea formed that I would be punished in any case and this was unpleasant. Then yellow and red again appeared and suspense and fear reasserted themselves. I thought that I would surely be punished now. The disc stopped at purple and I decidedly expected the shock. The fear remained while I awaited the pain. The latter was very intense and was accompanied as in other cases by a kind of confusion. I must mention the fact that I did not approach this experiment in the same spirit as former experiments. In the latter cases the fact that I had to react interested me and made me approach the experiment in a pleasant general emotional state, no matter what happened afterwards. In the case of this experiment, however, I am compelled to await the painful shock passively. This seems to give an unpleasant tone to my general state.

A further principle can be introduced in connection with a fourth disc, which is also so constructed that the *O*s can be entrapped regularly. This is that *O* is instructed to punish *E* for his (*O*'s) mistakes instead of the usual procedure being followed, namely the punishment of *O* by *E*. *O* then normally feels compassion for or sympathy with *E*—although sometimes shame and embarrassment occur. When we performed this experiment one *O* felt malicious joy at punishing *E* in a few cases. (Satisfaction at the usual correct reactions and exaltation at escaping the trap are particularly strong in this type of experiment whenever the *O*s make a special effort in order to avoid the necessity of punishing *E*.) The following disc can be used. A blue-green combination (*i.e.* half of the coloured strip blue and the other half green) which is preceded by a red is followed six times by a purple-black-brown combination. Once, however, black-purple-brown succeeds these warning-colours. The remaining divisions are painted other colours. Two measures for keeping *O*'s attention constantly fixed on the disc are contained in the latter; *viz.*, the insertion of red in a few positions without the succession of the combinations, and the irregular intervals between the purple-black-brown combinations. The *O*s are instructed to react as soon as purple-black-brown, which follows blue-green preceded by red, appears, but not in the case of black-purple-brown. The measures already mentioned as helpful towards entrapping *O*s can again be employed.

Compassion. (1) B. I did not feel in a very high state of tension because the fact that the warning-colours would appear set my mind at rest. My attention was chiefly occupied by watching for the warning-colours. I was caught in the trap, however, because I had not in the least expected it, partly since I had forgotten the trap and partly since the warning-colours seemed to me to be connected with the true combination. Besides this I was not clear as to my task and it seemed to me that this experiment was more complicated than former ones. Regretful irritation and an ashamed grin accompanied my discovery that I had been entrapped. At first I partially forgot that I had to punish *E*, *i.e.* thoughts concerning the punishment were only very vague. I supposed and hoped that it would not be necessary. Quite separate from former thoughts and emotions, there were other thoughts after the experiment, *viz.*, at the application of punishment. A very definite and strong emotion of pity for *E* of whom I thought as suffering for my mistakes and a really unpleasant shame and self-reproach arose. In short I extremely disliked punishing *E*.

(2) C. I began this experiment very confidently and was sure that I would react correctly. I also had a suspicion that the combination would appear soon. The appearance of red made me think this. I was not at all surprised when blue-green appeared and absolutely expected the reaction-combination. I was ready for reaction. When it appeared I pressed the key immediately and directly after this saw that I had fallen into the trap. I saw this even before *E* said so. I was very much disappointed. When I had to punish *E* I felt compassion for him in pain. I did not think of the fact that he was suffering for my mistake but was just sorry for him because he was being hurt.

(3) F. I watched the passing colours intently and noticed red passing a few times without being followed by the other colours mentioned by *E*. I thought that this formed part of *E*'s plan to entrap me. Then I thought that some of these colours might also have been brown and not red at all. I had forgotten at precisely which combination I had to react when black-purple-brown appeared and therefore I doubted whether it was the correct one. Then I reacted and was surprised when I heard that I had been entrapped. I remembered that now I had to punish the *E* for the second time in succession and I pitied him. I did not like to hurt him and I thought that he might easily think that I was making mistakes on purpose although this was not so. Indeed I sympathised with him. Then I thought that the appearance of the red colours I mentioned before and the trap really made it difficult for me to react correctly. I argued that I was not wholly to blame for my mistake and that *E* was partially to be blamed for it himself—he had made my task difficult. The compassion which I felt for him while I applied punishment was not so strong as on former occasions when I had only blamed myself.

Shame and Embarrassment. (1) C. I felt rather strained at the beginning of the experiment but not so much as before. I had more self-confidence. When the disc commenced moving I lost much of my confidence since I could not always distinguish the colours and thought that I might perhaps not recognise the correct one. I could not remember the first warning-colour. Then the combination blue-green appeared and I had a vague idea that it was somehow or other connected with the true combination. When I had allowed the true combination to pass unhindered owing to my uncertainty I felt sure that I ought to have reacted. I felt very much disappointed and strongly ashamed of myself. I expected *E* to say 'wrong' and winced at the thought that I would have to punish him. A painful embarrassment was present which I cannot describe further.

(2) D. I soon noticed that I had been entrapped. I had previously awaited the appearance of blue-green and it seemed to me to be practically certain that reaction would be necessary shortly after the appearance of this combination. I saw that my reaction was wrong immediately after I

had reacted and laughed embarrassedly. I heard *E* say 'wrong' and punished him. It seemed to me as if I should have noticed in time that the right combination was not exposed in the slit and that I had reacted half automatically and had not been attending properly. While I was punishing *E* I blamed myself for being too inattentive. The embarrassment was quite strong whereas there was only a slight unpleasant feeling when I had to punish the *E*. It was as if the embarrassment altogether predominated in my consciousness. I felt little affected by the punishment and also had the idea that *E* did not mind the shock very much. I do not remember thinking that I was punishing *E* for my mistake. I simply applied punishment automatically in these cases as a duty following upon wrong reaction. I no longer gave further attention to the application of punishment as was the case formerly when the thoughts about it made me pity *E*. The embarrassment accompanied the consciousness that I had reacted incorrectly.

Malicious Joy. A. I was not altogether concentrated on my task when red appeared. Its appearance made me expect purple-black-brown however. Still I was not quite ready to react when the combination appeared, and was entrapped. I had decided beforehand to attempt to react correctly without thinking of the fact that I had to punish *E* and without any recognised wish to avoid having to punish him. I do not remember thinking about the punishment when I saw with disappointment that I had been entrapped. A short time after I had noticed my mistake, I remembered for the first time that I had to punish the *E*. I do not remember that a definite feeling was present when I thought of this, for immediately afterwards my attention wandered to the disc which was again rotating after my mistake, and I thought that I should stop it first of all. I heard *E* say something and supposed that he meant I had committed an error. At last, just as I intended to stop the disc and to apply punishment to *E*, I felt a very clear emotion of malicious joy at the pain of *E*. There was also an impression as if I thought: "Aha, now you are going to get it." Out of the corner of my eye I saw him wince and this amused me somewhat. Then it occurred to me that I was pressing the button of the key rather too long and that it was not fair to punish him so immoderately. This thought was accompanied by an unpleasant feeling—I felt sorry for him. Then followed the idea that I must end the punishment and I immediately lifted my finger from the reaction-key.

Delight rather than satisfaction usually results at a correct reaction in connection with a fifth disc (which is also so constructed that the *O*s practically always react correctly under seemingly adverse circumstances), where the severity of the punishment is increased although not to an exaggerated and confusing extent. Whenever the *O*s are then punished unjustly (*i.e.* when their reactions are in reality correct) after they have avoided punishment several times by reacting correctly, anger is normally aroused. This only happens, however, if the *O*s recognise the injustice. The conditions are favourable for vexation to arise in connection with a disc of the same type when the *O*s are also punished unjustly but more leniently than in the former case although still painfully enough to be irritating. The disc can be coloured according to the following scheme. In four cases yellow and blue, and in four others red and green, succeed each other; each colour appears before the other of its pair in two out of the four cases. In a few other instances yellow and blue appear on the disc with another colour interposed. Simi-

larly red, another colour and green sometimes follow one another. Other sequences of colours on the disc are yellow succeeded by red or green, and blue succeeded by red or green. Finally there are cases of the succession of pairs of colours somewhat similar to the pairs mentioned first; *e.g.* yellow and purple, vermilion and green. The remaining divisions of the disc are painted other colours. The *O*s are instructed to react whenever a colour appears which was immediately preceded by its complementary colour.

Delight. (1) C. At the commencement of the experiment I was in a fairly high state of tension since I had been thinking of the renewed difficulty of correct reaction while my eyes were closed and *E* was changing the position of the disc in the Ranschburg. A very strong desire was present to retain my record of having made no mistakes so far in connection with this disc. I was afraid of losing my reputation this time. I saw yellow appear and realised with relief that the experiment would soon be over. I absolutely expected the correct reaction-colour. When blue appeared I felt slightly nervous. Then I saw the correct combination appear and imagined that I could see that it was nearly ready to disappear again. I hesitated for a moment but suddenly realised that the combination was correct and reacted. A fairly intense delight, or joy, and self-satisfaction arose and the disappointment which had first arisen, when I did not react directly, disappeared immediately. The delight became more intense when the *E* said 'right.'

(2) E. At the commencement I was not in the mood to keep the instructions clearly in mind. When a colour appeared at which I thought I ought to react I did so without realising plainly what I was doing. I then feared that I had reacted wrongly. A consciousness of punishment which would follow was also present. When I noticed that I was right I felt delight tinged with excitement. My former doubt seemed to be suddenly displaced by the complete consciousness of having reacted correctly. When *E* pronounced judgment this delight increased. It was quite strong. I also realised that punishment would not be applied and was glad that I need not endure any pain.

Anger (and Vexation). (1) B. There was a fair amount of tension which increased as the disc revolved. This tension, I believe, arose in connection with my desire to fulfil my task. I did not think of the punishment. When I reacted correctly I felt satisfied but I do not know of any relief. I felt some doubt as to whether I was right and then thought of the punishment with some anxiety. Then *E* suddenly punished me for no reason. This punishment which was given although I knew I was right filled me with a vexed kind of anger. At the same time I remembered that I had placed myself in *E*'s hands for experimentation and could expect things to happen. I think that my anger was at first slight owing to this idea. I must say that this angry feeling is much stronger now and then when I think of the unjust pain caused me and forget the other considerations.

(2) D. I was sure that I had reacted correctly and felt a slight satisfaction. I heard the *E* say 'right' and then felt the punishment. This seemed to me to be contradictory. I closed my mouth tightly and felt a distinct tendency to obstinacy. I said to myself: "But it *is* correct." In spite of former unjust punishments I still had the idea that my correct reactions meant avoidance of punishment. I could not resist feeling ill-tempered. I felt angry. After a while, however, I decided that I had to tolerate the pain and my anger subsided.

Shame, finally, can be aroused in a fundamentally similar manner. A disc is so constructed that it appears to the *O*s to be easy to avoid the trap, although they are nevertheless regularly caught in it. They then become clearly conscious of their own incompetence. Furthermore, only a moderate punishment is applied which, besides ensuring a clear consciousness of the mistake, contributes to the intensity of the shame by creating the impression that *E* was certain that *O* would be caught, pitied him and therefore did not wish him to be punished too severely. The following disc can be used. Five divisions are painted green with three dots and two dashes and one green with two dots and three dashes; in each case blue appears two divisions earlier. The other divisions are painted other colours. The *O*s are instructed to react when green with three dots and two dashes appears, and are told that it is preceded by blue two divisions earlier. They are also warned against the trap, and told that it can easily be avoided so that being entrapped will be very discreditable to them and lead to punishment such as is meted out to a little child. Here again the previously described additional measures for entrapping the *O*s can be employed. (Satisfaction, and even delight immediately after a trap and the alteration of the disc's position, is felt when the *O*s react correctly at a true reaction-position.)

Shame. (1) C. I did not concentrate sufficiently on the rotating disc at first and felt startled and nervous when I noticed my inattentiveness. It was unpleasant. I awaited the colours in a very high state of tension and absolutely expected the true combination. I gave no thought whatever to the wrong one. I had just pressed the button, however, when I saw that I had been caught. I felt disappointed and very much ashamed. The latter emotion increased when *E* said "wrong—trap" and punished me. It was the first time that I really did feel the shock as a punishment. Later the disappointment increased when I considered the fact that the reason why I make mistakes is that I regularly forget the wrong combination although just before the experiment I decide to remember it.

(2) D. I noticed that the disc was moving fairly rapidly and became slightly confused. Owing to the excitement and tension, I did not distinguish clearly how many dots and dashes there were on the green. I reacted without certainty but with the idea that the chances were in favour of a correct reaction. During my reaction I already began to doubt whether I was right and when the *E* confirmed my doubt I felt disappointed and very much ashamed. I had determined to react correctly and now my record was again broken. The shame was so strong that I felt the blood rise to my head. The idea was also present that I was making mistakes like a small child. It seemed very humiliating to me. I did not think of the punishment while the shame predominated, but when I felt the shock the thought arose that I was being punished like a little child and this still further increased my shame.

(3) F. I looked intently at the colours as they passed by and noticed that I felt highly strained. The question as to when the blue would appear was continually in the foreground of consciousness. The continual possibility of blue appearing next made me strained. I did not determine how many dots and dashes were present but roughly reckoned that the number was correct, and reacted trusting that I would be right. There was never-

theless a suspicion that I might have made a mistake. *E*'s judgment that I had been entrapped filled me with a very strong shame and disappointment. The shame was stronger than on the previous occasion because this was the second time that I had been entrapped in succession. *E* waited a moment before he punished me and this increased my shame. I felt quite relieved when he did not punish me, for *no* punishment in spite of the experimental conditions would have made my shame hard to bear.

Delight. F. I sat watching the colours intently and in a high state of tension, which increased as the coloured strips passed. My tongue seemed to hang out as I sat looking at the colours. After *E* alters the position of the disc I never seem to know what is going to happen and feel flurried. The task came clearly to mind, but I did not think of the punishment. A few of the colours drew my attention but I did not seem to be conscious of the colour of the other strips—I only realised that they were not the green for which I was on the lookout. When the green with the dots and dashes appeared I just thought: "You must press quickly." A strong delight made itself felt. I knew that I had reacted as I should. I was very glad that I had reacted correctly even after the alteration of the position of the disc.

We may add that the inductorium should always be put in action before *E* allows the disc to commence its movement—except in special cases when the absence of the sound made by the inductorium is introduced as a special variation of the experimental conditions.

Many other tasks and discs can be devised on the same underlying principles. Though not done by us, one can introduce further variations in the experimental method, *e.g.* by arranging a competition between *O*s as to who should react first. Pride and jealousy will probably be aroused under the experimental conditions which will ensue if the variation just mentioned is employed—especially if the application of punishment is adapted to the particular circumstances resulting from such an arrangement of the experiment.

Fundamentally this series of experiments, although by no means perfect, possesses the following characteristics which can be demanded of an effective psychological experiment. (a) It must be possible for the processes, which are being investigated, to be produced by *E* at his discretion and under certain definite conditions when these processes can readily be observed. (b) All the conditions under which the phenomenon that is being investigated appears should as far as possible be under the control of *E* in order to enable him to vary them at will. (c) The total situation must be simple and yet natural in the sense that it must show fundamental similarity to the situations of everyday life without the confusing complexity in the resulting state of consciousness which is in most cases characteristic of the latter.

The following facts support the statement that the experiments under consideration satisfy the above-mentioned demands to a great extent. The experiments were very efficient

in producing various emotions in *Os* in the psychological laboratory, even when (according to the evidence of the *Os* themselves) they had discovered what emotion was the object of the experiment concerned and attempted to resist the influence of the latter. Furthermore the nature of the emotion depends upon the varying of the conditions which are under the control of the *E*, such as: the task, the composition of the discs, the manipulation of the trap and the methods of application of the punishment. The emotions which arise are, according to the *Os*, quite natural in the sense of agreeing in quality with those of daily life, even though the contents of consciousness in connection with which the emotions arise are comparatively simple. The reproduction of various and irrelevant intellectual contents, which only produce confusing complexity, is prevented by utilising meaningless subject-matter. In this way complexity of emotional and intellectual processes is avoided and the total state of consciousness considerably simplified. Moreover *E* can to a great extent control the intellectual processes, which accompany the emotion produced, by a judicious use of the meaningless subject-matter, the task and the punishment. An important general advantage of this series of experiments is the brevity of the introspective periods which favour the reliability of the introspective reports. The duration of the period on which introspection has to be given, *i.e.* from the commencement of the rotation of the disc till *E* asks for introspection just after a correct reaction, a trap or a punishment, need never be more than five seconds and is very often less.

After the completion of experimentation the description of a somewhat similar kind of experiment came to my notice, namely, that of Levin.[8] A trial of this method convinced me, however, of the superiority of the one described above, especially in the control of the experimental conditions, in the relative certainty with which particular emotions can be intentionally produced, and in the absence, comparatively speaking, of irrelevant and disturbing elements.

[8]K. Levin, Eine experimentelle Methode zur Erzeugung von Affekten, *Ber. VII. Kongr. f. exp. Psychol.*, 1922, 146.

THE CRITERIA OF CONFIDENCE

By FREDERICK HANSEN LUND, Barnard College, Columbia University

Confidence, as an aspect of the processes of recall and recognition, of the attitudes of belief and disbelief, and of the various modes of perceptual life, has been a subject of growing interest to experimental psychology. It has been definitely shown that the recognition process is attended by varying degrees of confidence;[1] that the correctness of recognition tends to vary directly with the degree of confidence,[2] and that our belief-attitudes appear with varying degrees of strength, or varying degrees of confidence, assurance, or certainty.[3]

Since confidence is a common factor of our most fundamental experiences, it has proved a fruitful field of research. Studies thus far made, however, have been undertaken to determine whether there are different degrees of confidence, and to what extent the different degrees are correlated with the accuracy of recognition, of judgment, or of belief. A question of importance, however, is: "What are the criteria—the factors—which determine the degree of confidence?" It was to answer this question that the present study was undertaken. The results, though in no sense final, indicate the direction which the answer will ultimately take. First of all, however, let us consider, by way of illustration, a case in which the greatest possible range of confidence and recognition occurs.

Suppose one is waiting to meet a friend and finally a figure appears in the distance. For a while one is uncertain as to whether it is the friend. From the moment of uncertainty to the time when the person is clearly perceived, either as the expected friend or as some one else, there is a gradual recognition of the known characteristics in the one case or of the unfamiliar characteristics in the other. Every item recognized adds to one's confidence as to who the person is—whether this item be one of dress, gait, or stature—and every item becomes part of

[1]H. L. Hollingworth, Characteristic differences between recall and recognition, this JOURNAL, 24, 1913, 532-544; Experimental studies in judgment, *Arch. of Psychol.*, 29, 1913, 1-96.
W. C. Trow, The psychology of confidence, *Arch. of Psychol.* 67, 1923, 7-26.

[2]Trow, *op. cit.*, 33-37.

[3]Hollingworth, *op. cit.*, *Arch. of Psychol.*, 29, 1913, 1-96.
F. H. Lund, The psychology of belief, *J. Abnor. & Soc. Psychol.*, 20, 1925, 180-190.

the total context which finally constitutes the clear perception. The increase in confidence and clearer recognition is attained, not only through an increase in the elements that seem familiar, but in the increase in 'intensity' of these—that is, not only do more elements come into evidence as the person approaches, but every element becomes more distinct and confidence increases accordingly. Whether the increase in confidence and recognition proceeds regularly in this manner or changes suddenly in either direction will of course depend on the nature and sequence of the items which determine them.

If this analysis is correct it would seem to indicate that the feeling of familiarity appears as a result of these unitary and successive reactions occasioned by the complex stimulus—each adding to the summative effect which leads to the complete recognition. Confidence increases according to the number and intensity of the particular items or stimuli which initiate a familiar reaction. In other words, the feeling of familiarity arises from the fact that the different characteristics of the figure occasion the same perceptual and other reactions which have been made before in the presence of the figure, and that the *ease* with which these items lead over into reactions previously made determines the degree of confidence. In neural terms this would mean that the feeling results from the passage of a nerve current along a previously used brain path or across a closely bound synapse. Recognition will be prompt and confidence greater in proportion to the frequency with which the same neural pattern has been active previously under similar conditions, and in proportion to the intensity of the present stimuli, and the absence of stimuli which produce reactions which have a different tonal quality.

If the principle presented in the above discussion recommends itself as worthy of consideration, the question is how might it be put to test under experimental conditions. According to our analysis the degree of confidence which a situation arouses is conditioned by the ease with which it leads into a familiar pattern response, and that this is determined by the completeness of the situation and the absence of contradictory elements. A situation which varies in completeness and the presence of contradictory elements should not be difficult to construct except in so far as it is desirable to have the elements within the situation equally weighted for recognition value and thus devoid of previously established associations. For this purpose nonsense material would seem the best available. Fairly uniform combinations of two nonsense syllables, which would allow for articulation, and which, so far as possible, might be free from associative connections, were accordingly chosen for the experiment. This material is given in Table I below.

Procedure. The experiment was conducted by the usual recognition method with a fall tachistoscope and exposure cards upon which the nonsense 'words' of Table I were printed. The words of Series A were first presented at 10 sec. intervals and later, when presented for recognition, were given with those of Series B and C. After every presentation *S* was asked to indicate by 'yes' or 'no' whether the 'word' was recognized and to note the degree of confidence of his judgment according to the following scale: 0 (don't know); 1 (think so); 2 (fairly certain); and 3 (quite certain). The 'yes' judgments were marked plus and the 'no' judgments minus.

TABLE I

Nonsense words used in the experiment: Series A, the test material; Series B and Series C, the confusion materials with which Series A was mixed when presented for recognition. The letters in italics are those changed. The numbers in parentheses show the order in which the syllables were presented in the recognition test.

No.	Series A		Series B		Series C	
1	GATOR	(13)	GATO*N*	(9)	GAT*EN*	(6)
2	MAREN	(16)	MARE*R*	(12)	MA*RUR*	(8)
3	DULEN	(22)	*B*ULEN	(19)	*SA*LEN	(17)
4	KUNEL	(29)	*S*UNEL	(27)	*GA*NEL	(26)
5	AUMON	(30)	AU*L*ON	(28)	AN*L*ON	(23)
6	FABAS	(25)	FA*L*AS	(20)	F*EL*AS	(18)
7	SETIR	(24)	SATIR	(15)	*B*ATIR	(10)
8	BIDAR	(21)	B*E*DAR	(11)	*KE*DAR	(4)
9	PAMOR	(14)	PAM*I*R	(7)	PAM*IN*	(2)
10	CELOM	(5)	CEL*U*M	(3)	CEL*UD*	(1)

The *S*s were divided into 4 groups of 16, 34, 14, and 35 respectively. Groups I and II were given Series A *twice* before the final presentation with Series B and C. The sequence of the syllables in the recognition experiments is indicated by the numbers in parentheses. Groups III and IV were presented with Series A *once* only and then with the Series B and C in the *reverse* order of the numerals. The interval elapsing between the original and final presentations was no longer than necessary for the rearrangement of the exposure cards.

It will be seen that Series B duplicates Series A except that *one* letter has been changed in every 'word'; and that in Series C *two* letters have been changed. This 'duplication' was introduced in order to see whether there was any relation between degree of resemblance (or the degree of interference as the case might be) and degree of confidence, as well as to see whether a tendency to make false responses depends in any way on the number of similar elements. The control with respect to the order of presentation proved necessary as preliminary experi-

ments had shown that the nonsense words of Series A would be recognized more readily and with greater confidence if they preceded the 'words' of the other series to which they were similar.

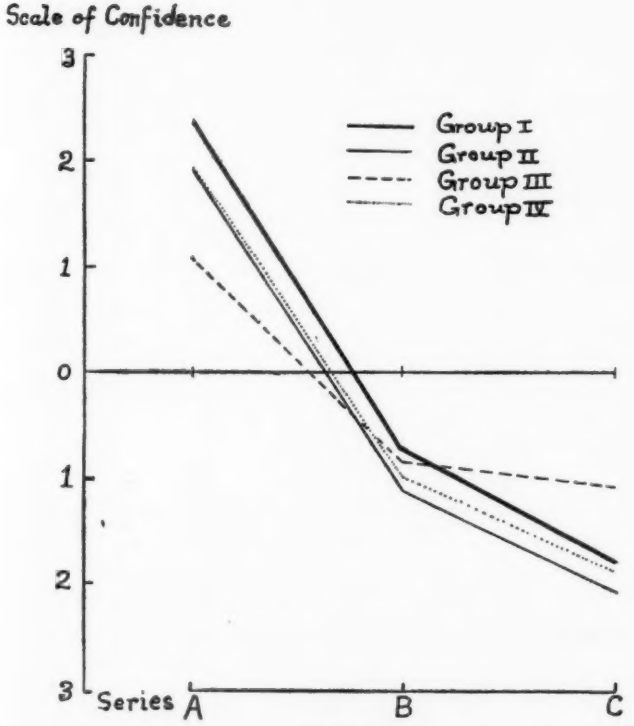


FIGURE I

Average ratings on the scale of confidence for the nonsense material of Series A, B and C by Groups I, II, III, and IV. The scale above and below the mid-line indicates degrees of confidence of the 'yes' and 'no' judgments respectively

RESULTS

Analysis. Figure I shows that two presentations were of little more value for recognition than one since there is no significant difference between the curves of the different groups. This brings out strikingly the importance of the order in the recognition series since the groups of *Ss* which were given two

presentations were given the recognition series in the order of the numerals, thus allowing the 'words' of Series B and C always to precede the 'words' to which they were similar.

Table II gives the average ratings of the 4 groups of *Ss* for confidence and the number of false responses. A response was considered false if it was 'yes' to any 'word' of Series B or C, or 'no' to any 'word' of Series A. Figures I and II present these results graphically. As shown by these tables and curves, the confidence ratings for Series A and Series C were higher than for Series B, and the number of false responses occurred with greatest frequency for the Series B.

TABLE II

Average ratings for confidence and the average number of false responses made by four groups of 16, 34, 14, and 35 *Ss* respectively. Groups I and II had two exposures of the presentation series (Series A). The recognition series for these two groups followed the order of the numerals (*cf.* Table I). Groups III and IV had only one exposure of the presentation series, while the recognition series followed the reverse order of the numerals.

Os	Series A			Series B			Series C		
	Ave.	S.D.	False Rep.	Ave.	S.D.	False Rep.	Ave.	S.D.	False Rep.
Group I (16 *Ss*)	2.3	1.3	1.1	−0.7	2.3	5.0	−1.8	1.6	1.7
Group II (34 *Ss*)	1.9	1.5	4.8	−1.2	2.1	8.5	−2.1	1.5	4.0
Group III (14 *Ss*)	1.1	1.5	3.7	−0.8	2.4	5.4	−1.0	1.9	4.4
Group IV (35 *Ss*)	1.9	1.7	5.8	−1.1	2.4	10.3	−1.9	1.8	6.2

Interpretation. (A) *The Reaction Hypothesis.* The degree of confidence is determined by the ease of redintegration,[4] that is, by the ease with which a present situation leads over into a familiar pattern response.

The results of the experiment confirm the analysis of the illustration used in the introduction of this paper. According to this analysis, confidence increased as the figure approached and this increase was due (1) to the gradual appearance of elements which combined to form perceptual reactions of a familiar sort, and (2) to the absence of contradictory and unfamiliar elements. It may be objected that in this experiment the range of elements within each situation (a nonsense syllable combi-

[4]The term 'redintegration' is used in the same sense as by Hollingworth (*cf. The psychology of functional neuroses*, 1920, 12-35; also, Meaning and the psychophysical continuum, *J. of Philos.*, 20, 1923, 436.), as referring to that type of process in which a part of a complex stimulus provokes the complete reaction that was previously made to the complex stimulus as a whole.

nation) was too limited. There can be no doubt as to the desirability of experimentation upon a wider range of material, or of the greater finality and conclusiveness of a more extensive investigation. It might be well to complicate the situation by introducing a variety of elements—color, form and other factors

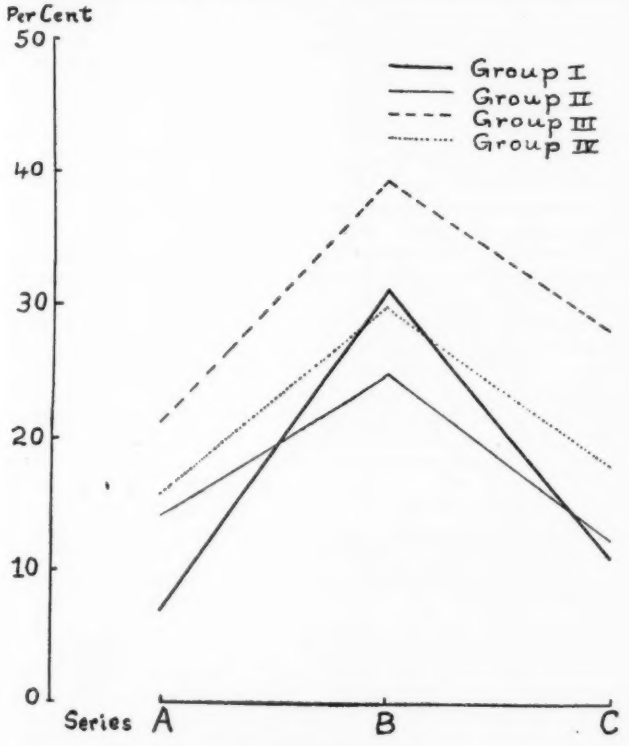


FIGURE II

Percent of false responses made by the *S*s of Groups I, II, III and IV in the recognition and confidence test employing the nonsense material of Series A, B and C.

which would make for differences in quality and intensity—thus approximating ordinary life situations. The difficulty of this would be that it introduces unequally weighted elements and tends to obscure the processes incident to recognition and confidence. Our data, however, permit us to say that at least

within the limits with which we have worked the principle we have set forth holds. The greater similarity between Series B and Series A occasions frequent false responses in the case of the former—much more frequent at least than is the case for Series C which is less similar and contains more contradictory elements (see Figure II). This fact is paralleled by Series B taking a position intermediate to Series A and Series C in the average ratings for confidence (see Figure I). Thus the number of false responses and the confidence ratings both indicate that Series B tends to redintegrate the pattern response of the 'word' in Series A to which it is similar, and that this tendency is great enough to be barely offset by the presence of the contradictory element. The degree of similarity with Series A being greatly reduced for Series C and the contradictory elements increased, the tendency toward an affirmed negative reaction is increased accordingly.

There is evidence, then, that not only is recognition attended by a given degree of confidence, but that this degree is dependent upon the degree of similarity which a present situation holds to some past situation, or the ease with which a present stimulus leads into a familiar pattern response. But it may not be clear in what sense we are dealing with a *reaction* mechanism.

Chiefly due to the behavioristic account, certain misconceptions are current as to what we are to understand by a reaction. According to the behaviorists, the term is applicable to visually observable movements and to the operation of such mechanisms as will inscribe themselves on a smoked drum. Such a position reveals the extreme sensuous bias with which this group of investigators are obsessed for facts taken in visually —a position which seems least excusable in psychologists. Psychologically, an individual's reaction in many cases of overt movements must refer to a process for which the outward manifestations are but symbolic indications. If I am interrogated as to the number of angles in a triangle and I make the familiar response, the 'stimulus,' or better, the antecedent event taking the form of a question, initiates the action of a neural pattern on the cortical level and the conscious states experienced with it. This is the significant reaction and precedes the overt behavior by which the answer is made manifest; but whether the overt reaction is a speech reaction, or the significant movement of raising three fingers, or writing the figure three, is not important so long as it takes some conventional and well known form. One cannot say that the latter are not significant, but which ever one is resorted to is an 'accident' as compared with the reaction preceding it and which is common to them all.

In this sense perception, recall, recognition and other aspects of the memory phenomena, as well as ideational processes in

their various forms, are reactions and point to characteristics of reactions as much as those which result in gross physical and outward manifestations.

According to our analysis recognition and the feeling of familiarity are characteristics of the states of consciousness arising from the re-arousal of previously active neural patterns and confidence is great or small according to the ease with which the situation redintegrates an earlier response—this ease in turn being determined by the number of familiar elements and the frequency and recency with which one has reacted to them. Briefly stated, recognition consciousness is a characteristic of reactions that are repetitions of former ones and attended by a degree of confidence proportionate to the ease of redintegration or the part-whole efficacy of the stimulus.

There are certain additional facts which may be summoned to the support of this account. The introspective observations of the subjects employed in the experiment furnish repeated instances of 'words' being recognized because they suggested some idea or associated idea which also occurred in the original presentation. The same report is made by Strong in his work on recognition.[5] Some of his *S*s were successful in their attempt to improve their record by allowing every item of the series to suggest some natural association. Müller reports similar observations in experiments dealing with recall and assurance of its correctness. When a nonsense syllable brought with it a word which it suggested in the process of learning this added to the *S*'s belief in its correctness.

From such evidence as well as from the observations of daily experience it appears that the cases in which recognition is most irresistible are cases in which an object recalls vividly and with fullness of detail the circumstances under which it was previously experienced.

Such instances give further emphasis to our statement of the mechanism of confidence and recognition: that an object is recognized because it reinstates previous reactions, perceptual or otherwise, and that the sense of having been seen before comes through the passage of a neural current through previously used brain paths or synapses.

(B) *Retroactive Effects upon Recognition and Confidence.* The observation that recognition and confidence for Series A was greater if it preceded Series B to which it is similar, is a fact which seems most readily explained as due to retroactive effects.

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[5]E. K. Strong, The effect of length of series upon recognition memory, *Psychol. Rev.*, 19, 1912, 447-462; The effect of time-interval upon recognition memory, *ibid.*, 20, 1913, 339 ff.

Apparently, interposing Series B had the effect of interfering with the connections and the neural set formed in the first presentation of Series A.

This interpretation does not accord with the findings of Müller who maintains that the difference between recall and recognition observed in the greater efficacy of the latter, is to be accounted for by the absence of retroactive inhibition in the recognition process.[6] This conclusion is based on an experiment in which in one case the presentation was followed by strenuous mental work and in another by a rest period. There was no difference in these two cases in the amount recognized. It is likely that Müller and his associates in this experiment were working too far above the threshold or even within the recognition span. However this may be, there can hardly be any doubt as to the presence of retroactive effects in recognition, or that the basis of recognition and recall, contrary to Müller's assertion, is different in any important respects. The fact that recognition is less efficient when a large number of objects is presented than when a smaller number, as well as the fact that a lapse of time between presentations makes a difference, should be sufficient to demonstrate retroactive effects.

Such effects are not peculiar to the type of memory phenomena usually considered under the head of recognition and recall. There is no reason why they could not be accounted for by *interference effects* of the same sort as occur in the card-sorting experiments when there is a change from one arrangement to another, or in the experiments of Fernberger on the influence of physical work on the formation of judgments in the weight lifting experiment,[7] or in those of Angell on similar phenomena in the field of audition.[8] In each case we are doubtless dealing with associative connections or neural patterns basic to certain forms of reactions which tend to disintegrate with the passage of time and the formation of new connections. To postulate basic distinctions between these processes and their interference effects, or to differentiate them as 'more physical' or 'more mental,' although in one case a portion of the gross musculature is involved and in the other it is not, appears as an unwarranted assumption.

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[6]Cited by Hillingworth, *op. cit.*, 532 f.

[7]S. W. Fernberger, The influence of mental and physical work on the formation of judgments in the lifted weight experiment, *J. Exp. Psychol.*, 22, 1916, 508-533.

[8]Frank Angell, Experiments on the discrimination of clangs for different intervals of time, this JOURNAL, 11, 1899, 67-79; also, Discrimination of clangs for different intervals of time, *ibid.*, 12, 1900, 58-79.

SUMMARY

The degree of similarity which a present situation has with a previous situation, and therefore, the degree of similarity which present perceptual and associative reactions have with previous ones, determines the degree of confidence. The ability of a present event to arouse reactions which carry with them the sense of having been experienced before, the ease with which present stimuli redintegrate a familiar pattern response, and the combined effect of elementary reactions afforded by a situation or the nature of the perceptual reactions to them as a group, are the determining factors or criteria of confidence.

THE MECHANISM OF CONSCIOUSNESS: THE PERCEPT ARC[1]

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In a preceding paper an analysis was made of the mechanism of the phases of consciousness that intervene between the external stimulus and the sudden appearance of perception. In this paper the preceding analysis will be extended to cover those phases of the conscious process that involve perception.

It will be convenient to state the general conclusion at the outset, namely, that the direct relations between perception and other conscious content may be represented by assuming an arc, the percept arc, which stands in the same general relation to the pre-sensation arc, as that arc, according to the Hughlings Jackson scheme, stands to the next (unconscious) arc below it. Figure 1 graphically represents this fact, heavy parts standing for the new arc and the lighter parts for the pre-sensation arc. The motor half of the assumed percept arc is only outlined as the facts are not at hand to complete it. Moreover this paper does not concern itself with this part of the arc.

For physiological, anatomical, and histological reasons which are familiar, the percept centers will be located in the so-called projection area, *i.e.* for cutaneous sensations in the area posterior to the Rolandic fissure (Brodman's areas 4, 3, 2), for hearing, inferior to the fissure of Sylvester (Brodman's areas 41, 42), and for sight, around the Calcarine fissure (Brodman's area 17).[2]

The first of these areas has received the most careful clinical study. For lesions in this area "the change in sensation is not shown as a loss to touch, pain, heat or cold, but as inability to recognize special relations, relative intensity, or similarity and difference in various test objects."[3] This is a rather large list of symptoms, the last two of which may be due not directly to the loss of the sensory center but to the destruction of the path from that center to the next higher center—that of images. In any event, the most striking effect of an injury to the *gyrus posterior centralis* is the loss of power to fit the pre-sensations into a standard frame of reference. Thus, in cortical injuries of this type, the loss of thermal discrimination falls on only that part of the scale where "thermal recognition habitually depends on some previously existing standard," while in the case of tactile sensibility, the patient may be able to feel the stimulus, but he can not relate it accurately to his space or to his standard of intensity. Thresholds for cutaneous sensations are, therefore, difficult to obtain in cases of cortical injury in this area.

[1]This is the second of a series of three articles on the 'Mechanism of Consciousness.' The first on 'Pre-Sensation' appeared in the October 1925 number of the Journal (36, 1925, 588-596).

[2]K. Brodman, *Vergleichende Lokalisationlehre der Grosshirnrinde*, 1909.

[3]H. Head, *Studies in Neurology*, II, 1920, 805.

All of this agrees with the much more extensive picture of the function and mechanism of this arc as deduced from introspection. The most satisfactory treatment of perception is found in the writings of the *Gestalt* school, beginning with Wertheimer.[4] According to certain of these writers, the perceived object is projected on a ground from which it is distinct. This is what gives it 'figure.' To perceive an object is, therefore,

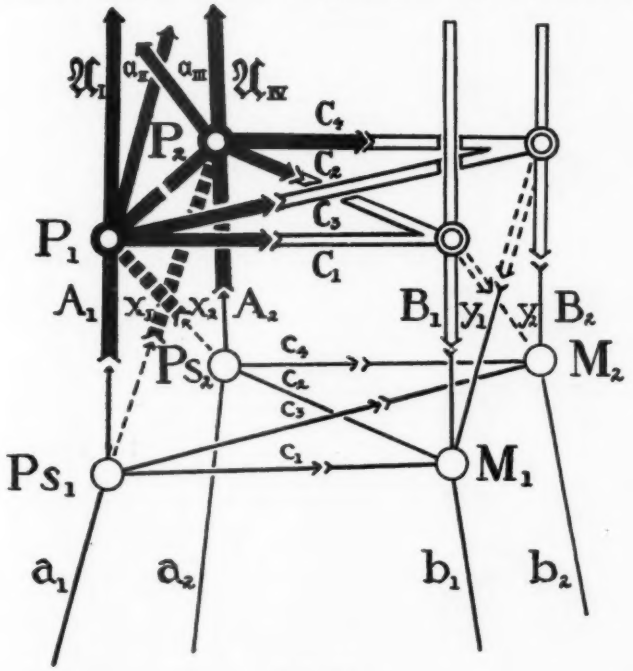


FIGURE 1
The mechanism of the pre-sensation and percept level of consciousness.

to relate it to a frame, which is ordinarily of the same modality as that of the perceived object. Thus when we see a faint light on a dark background we perceive the light set off from a residual visual phenomenon, *e.g.* blackness. Again, a sudden sound is perceived as contrasted to, and set off by 'auditory silence,' *e.g.* the ordinary level of faint noises usually present.

[4] M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 61, 1912, 161.

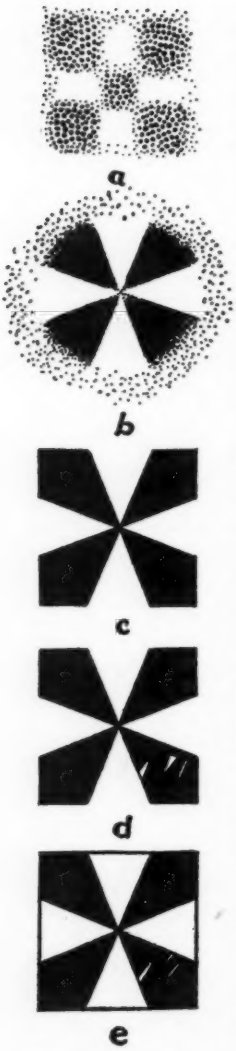


FIGURE 2

Let us investigate more in detail the nature of the 'ground,' which has the following introspective characteristics; it is non-focal, it possesses modality, it possesses no details, it is not spatially extended by itself but acts as a field, it has no outer boundaries, it is unified and it is disposed to fuse. The following are relevant observations.

"The stimulus took me unawares. I was saying to myself in voco-kinaesthetic terms: 'Statistical mechanics plus the quantum conditions ought to allow the calculation of the hydrogen molecule.' I was not prepared for the stimulus. There was dim awareness of other strain and discomfort (in the neighborhood of the eyes?) which was not part of the general voco-kinaesthetic strain commonly present in voco-kinaesthetic thinking. (It may have been due to contact with the 'blinders' on the instrument.) At the stimulus I was saying 'without knowing orbit.' The first I knew of the stimulus was hearing the faint s-s sound of the machinery. This seemed to be part of the 'it' sound of 'orbit,' giving an incongruous effect plus shock, *i.e.* kinaesthesis of quickly raised diaphragm, of movement of intercostal muscles, dry, cold feeling in the back of the mouth as due to a quick influx of air, kinaesthesis of raising eye-brows and focusing eyes. Then awareness all at once of gray visual 'feel,' not recognized as that, *plus* distinct activity of orienting the eyes. The strain activity around the eyes continued. The black-and-white rushed forward into focus, becoming localized in reference to the gray field in the center. While rushing forward the black-and-white were just feelings without figure. There were kinaesthesia and nausea, the details of which I can not remember. The instant the movement stopped the 'figure' was as presented in Fig. 2a. I had a voco-kinaesthetic image 'check' as if to say 'checkers.' This seemed to continue the previous voco-kinaesthetic activity. The image never really developed but seemed to be blocked off gradually by the figure which was now (Fig. 2b), changing rapidly into a cross on a white background (Fig. 2c). This white background had no border and the gray feel was not distinctly present, if at all. I became aware of the defects in the lower right arm (Fig. 2d) first as a check in the movement of the eye, then as a textural defect in the black arm; *i.e.* the cross looked as if drawn with a pen. The defects did not seem to be white spots; nor did

the white seem at a different focal distance from the black as did the white of the background. There were ocular strains, ending with kinaesthesis of winking. The gray sensation during the wink closed in, and at the end of the wink I was aware of the figure as four white wedges on black (at a different distance) and the background (Fig 2e). The black boundary lines were now part of the background, while previously they had belonged to the gray haze. The instant I attended to the background, however, the whole figure became a pattern of a black square and the gray background."

These characteristics of the background seem to be identica with those of pre-sensation, and in fact, it seems impossible introspectively to distinguish background and any pre-sensation of the same modality which is present at the same time. The two insist on fusing. It is, therefore, not unreasonable to consider the background phase of perceptual activity as activity of the underlying pre-sensation arc on which the activity of the percept arc is projected; though this must be considered as a tentative hypothesis only and one not absolutely necessary to the general theory.

The most cogent argument against this simple view is that apparently in cases of synaesthesia, in which, according to this theory, percept activity is stimulated by activity of pre-sensation arcs of a different modality, the background to the percept is, apparently, of the same modality as the percept, not the pre-sensation. If this is true, it requires that the modality of the percept activity determine the modality of the ground; how this can be done is not exactly known.

Confining our attention for the time to this underlying ground, it will be seen that the assumption which makes the character of the ground depend on induced or direct activity in the underlying pre-sensation arcs accounts for the unified nature of the background experience, all pre-sensation activity having a unitary quality (due to prevalence of fusion). It also accounts for its experience-filling character, including its lack of outer boundary. The background even of the visual modality having no outer edge, limit, or place, where the visual field becomes non-visual. Of course, color and light intensity of the visual field fade off at the edge of the field as defined by the effective limit of the retina; but always at the outer edge—look as sidewise as one will—there is a background, sometimes black, but a visual black, and identical with a black background which can be seen objectively. This lack of an outer edge to the percept background is what would be expected logically from the theory that boundary to any experience depends on the co-existence of activity of a lower level. Since the pre-sensation arcs are apparently the lowest level arcs whose activity can be analyzed by introspection, it is, therefore, necessary that pre-sensational experience should be unbounded.

Putting this same argument in a form which will appeal to the more epistemologically minded reader, it is a reasonable assumption that consciousness includes only conscious events. If the simplest ultimate unit or

atom of consciousness is the pre-sensation and all other conscious events can be analyzed into them; *i.e.* if it is impossible to be conscious of any non-pre-sensation, something not analyzable into pre-sensations and their compounds, it is impossible to be conscious of a non-sensational field which would serve as a limit to pre-sensations.

Internal limits, *i.e.* structure of percept background, seem at first thought common enough; but experiments with certain figures appear to show, though details can not be given, that consciousness of the detail in a background is perceptual. Whenever the observer is conscious of details in a background a new background appears in consciousness on which the details of the old background are projected. At the same time, the old figure becomes lost as such, becoming part of the new pattern, *i.e.* old figure—detail in old background, all being projected on the new background. It may be that something of this sort is responsible for the apparent lack of background of differing modality in cases of synaesthesia.

We turn now to figure-consciousness. The general theory has already been indicated. Figure-consciousness is assumed to be due to the activity of a higher arc superimposed on that of a lower. This statement is, however, hardly more than a description of the most obvious introspective characteristics of figure; namely, the fact that it is projected, is limited, but at the same time, is a spontaneous and a novel activity.

Just what are the precise conditions which lead to the stimulation of the higher arc is not entirely known. Possibly one condition is the nature of the previous activity in the pre-sensation level; *e.g.* the existence of a 'background' of the proper modality. At least, often when the observer is aware of a new stimulus of differing modality, the first content to appear is the pre-sensation of the new modality, then the figure. However, in my own case, I have not been able to consider this as a universal rule.

A more nearly universal condition is the co-existence of kinaesthetic pre-sensation or sensation which often seems to touch off the activity of the higher arcs. For instance, this seems to be the factor determining whether, in cases of binocular rivalry, an image from one eye or the other will dominate. The dominating impulse in my experience always being accompanied by kinaesthetic pre-sensation. It would seem, therefore, that when a group of impulses comes into the pre-sensation level they stand a greater chance of acting as stimuli for the percept arc when accompanied by kinaesthetic pre-sensation than when they come alone. This may be called the trigger-action of the kinaesthetic pre-sensation. It is a very prominent introspective fact.

However, it is hardly likely that the kinaesthetic pre-sensation would be the only 'trigger' pre-sensation and analyses have shown that, in reality, the action of kinaesthesis is not strictly that of a trigger; in fact, it usually shows its presence in the resultant percept activity—just as did the group to which it acted as an apparent trigger. Thus, a more accurate statement is that groups including kinaesthetic activity are more likely to act collectively or partially as stimuli for percept activity than more passive groups of pre-sensations. When put this way, however, it is seen that coördinate kinaesthetic activity can hardly be the sole condition for perception, for cases are known where other pre-sensations, not of the kinaesthetic type, act as 'triggers' both in the old and in the revised sense. Moreover, there is nothing constant about this behavior. On one occasion a given pre-sensation will act as a trigger, not itself appearing as a percept; on another it will act as part of a stimulating group and a third will fuse into the background.

The most promising suggestion is that the important factor controlling the appearance of percept activity is not the presence of some specific kind of excitant, 'trigger' or otherwise, or even the presence of previous activity of the lower level; but is the presence of a certain state of activity of the higher level. This is what would be expected from the general fact that the state of the highest arc involved is the controlling factor in all activity below it. From this point of view it would be expected that impulses from a lower level might break through the upper level if intense enough, no matter what the previous activity of that level might be, but that normally they would break through without loss or disarrangement only if the state of the synapses between pre-sensation and percept arcs were favorable; *i.e.* if passage of this gap was not inhibited by out-of-phase activity of the first percept neurone. This could only be if the previous activity of the percept arc bore a particular relation to that of the incoming sensational activity; that is to say, if the previous activity of the percept arc was 'favorable' for the reception of a particular grouping of impulses from the pre-sensation level.

This view accounts for one of the most marked features of figure consciousness, its dependence, both in character and occurrence on the previous activity of the centers involved which must be 'consistent' with it. One can hardly help being impressed on reading the elaborate introspection, as published by Wheeler and others, by the 'logical' nature of the process of perception. One event follows another in consciousness, as the events in a play, each with its 'logical' cause, the whole account having what may be called 'plot.'

The percept arc may be looked at, therefore, not as an automatic mechanism for converting impulse into motor activity, as in the uncontrolled pre-sensation arc, but as a more or less plastic mechanism where the incoming impulse tends, other things being equal, to move parts whose friction has been reduced by previous use, but where the impulses, if suitably related among themselves and to the previous state of the mechanism, will cause new combinations of behavior. We may therefore consider the percept-arc as a machine for adapting the behavior of impulses in accordance with its behavior relations to the previous state of the machine. It is therefore the nature of previous experience, both immediate and fairly remote, which controls what groups of impulses stimulate percepts. This makes percept activity an individual matter depending on such things as training and previous behavior. More fundamentally it makes percept activity depend on the condition that the incoming group possesses a relation to the previous state of the percept arc which will serve for the well-being of the animal in the phylogenetic sense.

For this reason it would not be safe to expect a mechanistic explanation to be able to fix exactly what must be the relation between activities so that a given percept activity would ensue. The best that can be hoped is to describe some of the main features and limitations of percept activity in a more general way.

Of these features the possession of limit, position, extent and shape of form seem the most characteristic. The first thing to be noted is that all of these features are relative, either to the field or to the parts of the percept. This holds for the most careful introspective analysis, it being impossible to find any new content corresponding to these factors. All that is found in a figure is pre-sensation plus the additional property of not fusing with other pre-sensation, as it would if it were activity of the pre-sensation arc alone and possessing relations to other pre-sensation activity. In other words, a figure on its background (and, of course, a tone on its background) is not to introspection an essentially different sort of thing from a pre-sensation, excepting insofar as it does not fuse with other percepts or pre-sensations and has conscious relations to these other activities. A perception, therefore, may be considered as in essence the same type of activity as pre-sensation but repeated on a different level and without the tendency to fuse spontaneously to give a single diffuse activity which characterizes the uncontrolled activity of lower levels. Its most pronounced characteristic is its dependence for character and existence on the possession of certain relations to the coördinate activity and state of its own level and to that of lower levels.

That this is the case, and that perception involves consciousness of relation; *i.e.* that perception is percept-arc activity in relation to its parts and to the activity of its ground, can be detected by direct introspection of the best quality and from even those of second quality by noting the transitional, pseudo-independent and universal characteristics of perceptions as contrasted with pre-sensations. These characteristics, as has been shown,[5] mark relational activity within consciousness. Even without minute introspection, the fact that these special qualities of perception are called position, shape and extent is an indication of their relational nature. In short, perception is a primitive form of judgment and must be represented by a sentence; *e.g.*, "This activity has a certain relation to the ground." The percept is, therefore, not the activity of the percept-arc alone or the activity of the ground but the two in a definite relation—just as a sentence is not the subject or object alone, but subject so related to the object.

[5] F. R. Bichowsky, this JOURNAL, 34, 1923, 231.

Now it is realized that this position does violence to the ordinary conception of the atomistic psychologist that all there is in consciousness is sensations. If he would accept the evidence previously given, he would see that, though it is true that relations by themselves have no objective existence (while pre-sensation apparently does possess existence apart from relations), it by no means follows that relations can safely be neglected, even in accounting for the objective character of the related nexus; for it is a matter of logic and common sense that groups largely depend, in their objective characteristics, upon the relations between their parts. Though there may have been a time for a purely analytical psychology, which merely described the atoms of which the conscious state was composed, such a time has long since passed.

The most interesting empirical reason for the point of view which considers the relation between conscious elements important, comes in the detailed study of the relations involved in perception from the dual point of view of introspection and physiology. It turns out that the relations found by introspection are almost, if not entirely, identical with the relations between the underlying physiological activity as far as these can be discovered.

To introspection the relation of extension seems to be of the two following types: (1) 'Voluminous' extension. Consciousness of extension of this type is independent of consciousness of boundary, shape, or size. It is difficult introspectively to distinguish the 'voluminous' percept from the pre-sensation which also has a very similar voluminous feel; but if the quantity of the stimulus is changed, the pre-sensation is normally modified in intensity, while the percept activity, which is very difficult to attend to, merely changes in 'voluminousness.' It feels bigger, as if more units were stimulated, which, of course, is an adequate description of the probable physiological fact. (2) Metric extension, involving also consciousness of shape and position. Such percepts of size involve the consciousness of the relation between parts and between parts and field.

Consciousness of location seems to be of two types: (1) Location with reference to the field. In my own case, consciousness of location of this type always involves, besides the visual background, kinaesthetic strain of the eye muscles. The system of coördinates used by me in location is, in fact, not a visual set, but is the magnitude of kinaesthetic strains. Without kinaesthetics of this type I am unable to place an object in either two or three dimensional visual space. This means physiologically a constancy of relation between the strain in the eye-muscles and visual stimulation of a particular part of the percept arc. It is this strain in this relation to visual activity that I find in cases of localization. (2) A second type of localization is also possible; *i.e.* relative to some other part of the object, but this can best be discussed in connection with perception of shape.

Perception of shape seems to be quite independent of perception of location. In the case of vision the mechanism is the same; *i.e.* the consciousness of the amount of eye-strain in moving from the one point to the other. The difference is that the standard of muscular strain is not the fixed tonus of the muscles, but the particular state of strain the eye-muscles were just in. Perception of motion has not been satisfactorily covered in this investigation. What seems to be the case, though this is hardly more than a suggestion, is that perception of motion is shape-perception inverted; that is to say that in perceiving motion instead of being conscious of strain due to real motion of the eye, as in case of consciousness of shape, the observer has a succession of images without coördinated eye-movement, and that, because of the habitual connection of movement of image with kinaesthesis, kinaesthetic perception is stimulated. The motion is thus a kinaesthetic, not a visual percept.

Of course, the information given above concerning the nature of the relations needed to account for the facts of location, shape, size and motion, is applicable to only certain of the percept relations, and is, moreover, none too firmly established. However, the assumption that the relational elements in perception do not require any new type of introspective 'substance,' but just are the relations which physiologically exist between the various activities of the arcs involved, is so attractive that it may be tentatively assumed, particularly as it is but a natural extension of the ordinary view that the psychological picture of things must be consistent with the underlying physiological mechanism and because there seems to be no other explanation of the fact that relations among perceptions do not appear to involve new conscious elements.

THE TACTUAL PERCEPTION OF FORM

By M. J. Zigler and Kathryn M. Northup, Wellesley College

This study endeavors in a somewhat preliminary and schematic way to throw light upon three related problems: (1) the degree of accuracy with which observers can render judgments as to the form, or shape, of certain common geometrical figures applied to the skin with vision excluded; (2) the psychological processes involved in the perception of tactual form and in particular the more salient features by which the forms of these figures are primarily apprehended; and (3) the rough determination of that magnitude of stimulus around which the threshold for the perception of these forms lies.

We used 5 straight-sided plane figures as stimuli: square, equilateral triangle, right-angled triangle, diamond, and hexagon. In our initial work, these were cut from cardboard and mounted upon a wooden handle which was convenient for manual application.[1] In every one of these figures the main dimension (the sides of the square and diamond, the altitudes of the triangles and the separation of the parallel sides of the hexagon) measured 20 mm. In subsequent work, and in particular for the performance of the third task, we used a series of 5 stimuli for every one of the figures named above. The main dimensions of the 5 members of every series were 5, 10, 15, 20 and 25 mm. These were cut from hard rubber. The threaded end of a brass handle could be screwed into a hole in the mid-point of every one of these stimuli. In order to insure a fair uniformity of intensity of application, the central rod of the handle passed through and worked against a small spiral spring, which was enclosed in the housing of the handle. A small slot running lengthwise in the wall of the handle-housing allowed an indicator, which was attached to the central rod, to protrude so that *E* could apply all stimuli at a uniform and determinate intensity. The intensity of application used was roughly 500 grm.

The *O*s include 3 undergraduates in advanced laboratory classes, Miriam Arrowsmith (A), Helen Land (L), Kathryn Ward (W) and the senior author of this study (Z). The stimulus was applied to the volar surface of the left forearm at a point about midway between the wrist and elbow. The stimulated member was passive throughout the application and was at all

[1] M. Carnes and L. C. Shearer, this Journal, 27, 1916, 417 ff., demonstrated that careful manual application insures practically as good results as mechanical application.

times screened from *O*'s eyes. The instructions read: "After the 'ready' signal a tactual stimulus will be applied to the volar surface of your forearm. You are to observe carefully and report the shape of the tactual impression and all observable criteria upon which your perception of shape is based." None of the *O*s, except Z, had any knowledge of the figures used. The results of our work upon the three questions follow.

TABLE I[2]

O	CLASS	Square	Equil. Triangle	Right Triangle	Diamond	Hexagon
A	Accurate	25	25	42	12	
	Confused	33	25	17	12	
	Inaccurate	33	33	29	54	92
	No Shape	8	17	12	21	8
L	Accurate	37	25	29		
	Confused	46	29	29	12	
	Inaccurate	12	42	33	83	100
	No Shape	4	4	8	4	
W	Accurate	21	29	37		
	Confused	21	12	21	8	
	Inaccurate	50	50	37	83	92
	No Shape	8	8	4	8	8
Z	Accurate	29	29	54	50	8
	Confused	8	12	33	12	12
	Inaccurate	46	46	8	29	71
	No Shape	12	12	4	8	8

I. THE ACCURACY OF TACTUAL PERCEPTION OF FORM

After a short preliminary period of training the 5 cardboard stimuli were presented in haphazard order to every *O* 24 times to ascertain the accuracy and definiteness of tactual perceptions of form under our conditions of experimentation. The results of this work are tabulated below in four classes: (1) Accurate Perception: form was accurately perceived and reported without qualification; (2) Confused Perception: uncertain perception where *O* reported the true form of the stimulus as well as an incorrect form, but was unable to say positively that a mature and determinate form was perceived; (3) Inaccurate Perception: the report of a form different from that which was applied; and (4) No Perception: where the tactual impression failed to take an observable form. Table I shows the percentages of these several kinds of perceptions given by all *O*s and for all figures.

[2]The percentages in the table are not carried beyond 2 places. Where a fraction exceeds ½ a unit is added, otherwise the fraction is dropped. The dropping of small fractions accounts for the loss of a unit in several columns.

The relative fewness of accurate perceptions is one of the outstanding results. Only one *O* secured as high as 50% accuracy of perceptions, and this was Z, who knew what stimuli were being used, so that he was more likely than the other *O*s to limit his judgments to the 5 figures used. The other *O*s were much more inclined to give a more extended variety of forms, such as: oval, circle, oblong, half circle, straight line. Three *O*s did not mention 'hexagon' throughout the experiment. It was perceived as such only by Z who worked with partial knowledge, and by him very infrequently. It usually gave rise to a perception of square, circle or oblong. The diamond also was difficult for several *O*s. It was reported by them very rarely and was usually confused with the triangles. The right-angled triangle, except in the case of one *O*, secured the highest percent of accurate judgments. The hypotenuse connecting the two acute angles and running diagonally across the forearm constituted a fairly dependable landmark for the accurate perception of this form.

Another item of interest is the fact that, in spite of the instruction to observe the shape of the tactual impression, all *O*s were often unable to perceive any shape at all, not even enough to warrant a guess—and this for practically every stimulus. The vague and indefinite perceptions are not confined to the fourth class in the table. The instructions demanded a report of shape, so that the *O* was inclined to guess even when the impression of form was so vague that only a pressure blur was perceived.[1] These guesses fell into the inaccurate and confused classes; consequently, the perceptions in both of these classes may be taken as instances of indefinite perception of shape.

To assure ourselves that the stimuli were not too small for the facile perception of shape, we next ran through a brief series of experiments in which the main dimension of the stimuli was 35 mm. The outcome of this work, in spite of possible improvement as a result of practice, was, in the case of every *O* and for every stimulus, less (and in most cases markedly less) accurate than in the previous work. Because the larger stimuli were applied necessarily to a convex surface, they did not make so definite an impression along the contour of the stimulated field.

The most frequently confused and inaccurate judgments for the several stimuli were, in order of frequency: *square*—oblong, circle and oval; *equilateral triangle*—other triangles, semicircle, diamond and square; *right-angled triangle*—other triangles, diamond, oblong, square and oval; *diamond*—triangle, oval, oblong and square; *hexagon*—square and circle with about equal frequency.

II. Qualitative Aspects of the Tactual Perception of Form

When form is most clearly given, tactual processes alone carry the meaning of the particular shape. A definite and complete form, however, was the exception rather than the rule in our experiment. The more typical reports of all *O*s show that shape is not usually mediated by tactual processes only. Usually certain salient and discrete features of a figure, *e.g.* three disconnected angles of the equilateral triangle, two acute angles of the diamond, two straight parallel sides of the square, were definitely given in terms of sensory tactual processes but the incorporation of these salient features into a meaningful shape was as a general rule carried in imagery. Visual imagery was most frequently reported but sometimes the completion was made in something like a tactual tied-image, the *O* asserting that parts of the figure outline were not given in sensory tactual processes but in tactual imagery.

The salient features[3] of the several figures most frequently mentioned, given in the order of frequency, were as follows. *Square:* (1) two parallel sides, usually those running transversely to the arm; (2) pressure pattern of vague contour but having dimensions perceptually equal transversely and longitudinally; (3) two or more right angles. *Equilateral triangle:* (1) three acute angles or points; (2) an acute angle with sides of variable length and a straight line on opposite side of figure; (3) two sides and an angle or point; (4) three sides. *Right-angled triangle:* (1) a right-angle or point with hypotenuse running diagonally across the arm on opposite side of field; (2) two acute angles and hypotenuse; (3) three sides with hypotenuse most obtrusive; (4) elongated pressure field pointed at one end and gradually wider at the other. *Diamond:* (1) two acute angles, the sides of which diverge so as to make a wide pressure area with unclear outline in center of field; (2) an elongated field with two points at extremes and a blurred area between; (3) four points or angles with completed or uncompleted sides. *Hexagon:* this was not identified often enough to allow us to speak of a mode by which it was generally apprehended. Most confusions of this figure were with the square and circle. In the several instances when it was reported accurately by the one *O*, a field of equal dimensions longitudinally and transversely but lacking long straight sides or right angles was perceived.

From the foregoing it is patent that acute angles are more potent landmarks for the tactual perception of the form of figures than side-lines, while the side-lines are more likely to be

[3] In an earlier study (this Journal, 31, 1920, 273 ff.) the visually salient features of such figures were pointed out.

apprehended and to figure in the realization of the form of a figure than obtuse angles, and quite as likely as right angles. Acute angles are perceived much more definitely than right and obtuse angles. Because of increased sensitivity, the parts of the figure applied toward the wrist were generally more clearly apprehended than the parts falling more proximally.

As to whether form is mediated primarily by peripheral or by central processes, our results do not shed very conclusive light. It is apparent that central factors are involved, in view of the fact that imagery is employed to complete the form of a figure incompletely given sensorily. But there were a few instances when *O* asserted positively that the form seemed completely given in sensory tactual processes. There were also a number of reports to the effect that the realization of sensory pressure and of a pressure field having definite outline were temporally disjoined. In all such cases, the apprehension of a 'pressure blur' preceded the realization of a pressure having a particular shape. But this temporal disjunction form of report was offset by reports, not nearly so frequent, that the perceptions of pressure and of shape came simultaneously. There were frequent instances, too, when in spite of instruction to observe form, *O* could only report a 'pressure blur,' as though some factor, perhaps central, failed for some reason to appear. The apprehension of pressure was quite generally more definite and unfailing than the perception of pressure having such and such shape. It is conceivable that our filled figures may at times have hindered or delayed the perception of shape, and that outline figures might have enabled us to determine, had time permitted, with more assurance the immediacy and the definiteness of the apprehension of shape.

Another indication of the possibility of central coöperation in the perception of form is the fact that all *O*s showed more or less pronounced daily sets for particular forms. On a given day the *O* would report one form much more accurately than the others and at a later sitting the same *O* manifested a similar pronounced tendency toward another form. On the same day, however, the *O* would also report the favored form for inappropriate figures. The temporary central predisposition seemed to tend both toward the accurate perception of the particular form for which a temporary set was assumed and toward the realization of that form even if a different stimulus was applied.

III. The Approximate Threshold of Size for the Perception of Form

The limen of form of several geometrical figures has already been determined by Major.[4] However, several points of differ-

[4] This Journal, 10, 1898, 143-147.

ence between his work and our own must be noted, if comparison is to be made. Major stimulated more sensitive parts of the skin surface, employed a method involving knowledge, and used only one of our figures—the equilateral triangle. He expresses the opinion that a procedure without knowledge should give "practically the same" limens as the procedure with knowledge.[5]

We arranged series of hard rubber stimuli of every one of our 5 figures for the purpose of making our determinations. In every series there were 5 stimuli having main dimensions, taken as given above, of 5, 10, 15, 20 and 25 mm. These were presented in haphazard order, the and *O*s were instructed to name the form of the impression occasioned by the stimulus. Table II

TABLE II

Form*	Judgment	Dimension 5 mm.	10 mm.	15 mm.	20 mm.	25 mm.
Square	Accurate	0	7	13	25	20
	Confused	0	0	25	33	40
	Inaccurate	0	13	28	30	23
	No Shape	100	80	33	12	17
Equil. Triangle	Accurate	0	10	25	27	22
	Confused	0	8	30	33	35
	Inaccurate	0	20	33	33	40
	No Shape	100	62	12	7	3
Right-Angled Triangle	Accurate	0	8	23	30	22
	Confused	0	12	28	27	35
	Inaccurate	0	18	30	33	33
	No Shape	100	62	18	10	10
Diamond	Accurate	0	3	13	23	25
	Confused	0	3	20	30	20
	Inaccurate	0	8	33	32	35
	No Shape	100	85	33	15	20

*Results for the Hexagon omitted because it was reported by one *O* only (Z), and by him only 5 times.

embodies the aggregate percentages of accurate, confused, inaccurate, and failure judgments of form given by the 4 *O*s. The results for the hexagon were again as unsatisfactory as in the previous work. It was reported correctly by only one *O* again, and all told in 5 instances only.

The stimuli of 5 and 10 mm. quite generally gave rise to the perception of a mere point or a blunt point of shapeless pressure. In the few instances when the forms of the 10 mm. stimulus were correctly reported, the shape was described as very indefinite and the judgment as essentially a guess. The minimal magnitude of stimulus for the passive apprehension of shape in the region stimulated seems to fall between 10 and 15 mm.

[5] *Idem*, 146.

The optimal dimension seems in general to be 20 mm. The accurate judgments of the 10 mm. stimulus in every case amounted to less than 50%, and those of the 15 mm. stimulus in every case to more than 50% of the accurate judgments of this optimal stimulus magnitude. Hence our conclusion that the liminal magnitude falls somewhere between the 10 mm. stimulus and the 15 mm. stimulus.

Conclusions

(1) Tactual form is not so definitely and unfailingly apprehended as tactual pressure when vision is excluded and the member kept passive during stimulation. Tactual pressure is invariably perceived, while tactual form is not definitely given in a considerable percentage of cases, and is not apprehended at all in an appreciable number of instances.

(2) The realization of form is minimal in cases of 'mere pressure blur,' where pressure totally devoid of outline or highlands of pressure boundary is apprehended; and maximal where the completed contour of a figure-form is given at all points in terms of the tactual impression. Between these extremes are intermediate degrees of definiteness of form, where salient topographical features of the tactual impression of a form are perceived in isolation (the contour being broken between these tactual highlands) that must be completed either in visual or in tactual imagery or in both to occasion the perception of the particular form.

(3) The stimuli of common geometrical figures must have a main dimension of 12 to 15 mm. to give rise readily to a tactual impression of determinate shape, if stimulation is made on the middle of the forearm. Impressions occasioned by stimuli of smaller main dimension are described as 'blunt points without contour,' 'shapeless pressure,' or 'mere sensory pressure stimulation.' The optimal main dimension for the mid-forearm by our method of stimulation is somewhat larger, about 20 mm.

THE EFFECT OF COLOR ON APPARENT SIZE AND WEIGHT

By Carl J. Warden and Ellen L. Flynn, University of Wisconsin

The present study is a preliminary attempt to determine the effect of color and arrangement on the apparent size and weight of a series of cartons, identical in actual size and shape, and differing only in the factor of color and serial position. The first experiment is concerned with the illusion of size (color-size illusion); the second with the illusion of weight (color-weight illusion).

The cartons used in the study were of cardboard, rectangular in form, measuring 16.5 cm. high, 9 cm. wide and 5.5 cm. deep. Eight of these boxes were used. They were covered respectively with red, yellow, green, blue, purple, white, grey and black Hering papers. The paper was stretched tightly and was so cut that no seams appeared on the sides toward *S*.

Inasmuch as the primary interest in the problem was practical, being connected with the display of merchandise, an attempt was made to duplicate the conditions of retail trade. The cartons were placed 5.5 cm. apart in a glassed light oak case which stood 4.5 ft. from the floor. They were placed directly against the back wall of the case, which was 4 in. from the glassed front, with the wider side (9 cm.) toward *S*.

The first serial arrangement of the cartons was made by chance. The succeeding arrangements, however, were obtained by a systematic rotation of the boxes from left to right so that every one became the initial member of the series. The immediate context of a given box, that is the color of the carton to the left and right, remained constant under all the arrangements except when it was either the initial or final member of the series. The 8 arrangements of the colored boxes were as follows:

Arrangement	Colors
A	Gy, Y, B, W, Bk, G, R, P
B	Y, B, W, Bk, G, R, P, Gy
C	B, W, Bk, G, R, P, Gy, Y
D	W, Bk, G, R, P, Gy, Y, B
E	Bk, G, R, P, Gy, Y, B, W
F	G, R, P, Gy, Y, B, W, Bk
G	R, P, Gy, Y, B, W, Bk, G
H	P, Gy, Y, B, W, Bk, G, R

Everyone of the 8 arrangements was ranked by 6 college students—3 men and 3 women. The final score for a given color is therefore the average of 48 rankings.

Experiment I. The purpose of Experiment I was to determine the extent to which the apparent size of a carton was a function of its color. The order of the boxes was rotated so as to eliminate the effect of specific arrangement.

S was handed a card on which had been typed the names of the colors in the order corresponding to that in which the colored cartons were arranged. He was then instructed to indicate by numbers opposite the colors the relative sizes of the different cartons. The fact that the cartons were identical in size was kept from the *S*s. The experiment was, from their point of view, purely a test of size discrimination.

TABLE I

The Effect of Color on the Apparent Relative Size

Color	ARRANGEMENT								Ave.	S.D.
	A	B	C	D	E	F	G	H		
Red	4.5	4.2	3.9	4.8	5.0	4.5	5.0	3.2	4.4	.47
Yellow	2.7	2.8	4.5	6.0	4.8	4.7	6.9	6.5	4.9	1.33
Green	4.2	4.2	3.7	3.3	5.3	5.8	4.8	3.0	4.3	1.06
Blue	4.5	4.2	4.7	4.8	6.5	5.5	4.3	3.7	4.8	1.21
Purple	6.5	5.3	4.2	3.0	3.0	3.2	3.2	5.3	4.2	1.30
White	2.2	3.0	4.3	5.7	5.8	6.7	6.0	5.7	4.9	1.57
Gray	4.2	6.2	5.7	4.7	3.2	2.5	3.5	6.3	4.5	1.46
Black	6.8	5.5	4.2	4.2	3.2	4.8	4.0	2.0	4.3	1.46

The results of the experiment are presented in Table I. It is evident that the color-size illusion is present to a marked degree under every arrangement. The amount of the illusion, however, varies considerably from one arrangement to another. For example, the rankings in Arrangement A vary from 2.2 for white to 6.8 for black; while in Arrangement C rankings vary only from 3.7 for green to 5.7 for gray. Almost as great a difference in ranking occurs in Arrangements D and H. It is clear, therefore, that the amount and definiteness of the color size illusion is in part a function of the specific arrangement of the colors.

By referring to Table I it will be seen that the apparent size-value (relative) of a given color varies with the serial arrangement. For example, the average rank of yellow varies from 2.7 in Arrangement A to 6.9 in Arrangement G; White, from 2.2 in Arrangement A to 6.7 in Arrangement F; while red, which maintains the most stable rank position of all, varies from 3.2 under Arrangement F to 5.0 under Arrangement E.

When the rank values for every color throughout the 8 series are combined, the differences between the averages so obtained are so slight as to be insignificant. The standard deviations (S. D.) show that these averages have little or no reliability. The size-value of a color seems, therefore, to be dependent upon the specific arrangement.

While apparently a function of the serial arrangement, the color-size illusion is not due primarily to the contrast effect of the adjacent colors, for as will be recalled, the immediate context (preceding and succeeding color) was the same throughout

TABLE II

The Effect of Color on the Apparent Relative Weight

Color	ARRANGEMENT								Ave.	S.D.
	A	B	C	D	E	F	G	H		
Red	6.2	3.8	3.8	5.7	4.5	5.0	5.2	4.7	4.9	.51
Yellow	3.3	3.3	4.5	4.5	3.7	2.0	2.7	4.3	3.5	.98
Green	5.2	4.7	3.7	4.5	3.8	4.3	3.0	3.2	4.1	.32
Blue	3.8	4.7	6.2	3.2	4.5	4.8	5.3	4.8	4.7	.60
Purple	4.3	6.2	4.2	5.5	5.5	2.0	6.5	4.5	4.8	1.07
White	3.3	2.8	4.3	5.3	4.2	1.7	1.0	2.2	3.1	1.36
Gray	4.7	3.8	6.0	3.2	5.5	5.2	4.3	6.0	4.8	1.13
Black	6.3	6.2	4.2	2.8	5.5	7.0	8.0	6.5	5.8	1.46

the 8 arrangements except when a given color was the initial or final member of the series. No greater consistency in the relative ranking obtained at the end positions than obtained at the other 6 positions in which the immediate context did not vary. Furthermore, no general uniformity appears in the data as to the effect of the end positions on light and dark colors.

The apparent size-value of a color does not under our conditions, depend upon its own intrinsic quality or upon the contrast effect of its immediate context. The illusion must be dependent upon the specific arrangement of the series taken as a whole, that is, upon the *ensemble* of contrast effects produced by the total series. The data offer, however, no basis for an analysis of the specific factors involved.

Experiment II. In Experiment II an attempt was made to determine the extent to which the apparent weight of the carton was a function of its color. The arrangement of the cartons varied as in Experiment I, and the general procedure was also the same.

The results are given in Table II. The color-weight illusion, as will be noted, varies widely from one arrangement to another. For example, the rank values vary from 1 to 8—the greatest possible variation—under arrangement G; while the variation extends only from 3.7 to 6.0 under arrangement C. Similar comparisons can be made from the table. The amount and definiteness of the color-weight illusion is thus in part also a function of the specific arrangement of colors.

The data indicate, however, that the apparent weight-value of a given color is not wholly dependent upon the specific serial arrangement. Most of the colors have a consistent ranking that holds throughout the experiment. The difference between the average rankings show, in most cases, a relatively high degree of reliability. White, gray and black show a fairly consistent tendency, regardless of serial position, to form a series of qualities in which the color-weight illusion increases. The colors yellow, green, blue, purple and red form a similar series, although the rankings of blue, purple and red are not markedly different. It is not possible to determine, from the data gathered to what extent the color-weight illusion may be due to the contrast effect of the immediate context which remained constant in 6 of the 8 arrangements.

Conclusions. The results of these experiments may be summarized as follows:

(1) The color-size illusion, under the present conditions, did not depend on the intrinsic quality of the color nor upon the contrast effect of the immediate context, but, as would appear, upon the specific serial arrangement.

(2) The color-weight illusion was greater than the color-size illusion for this series. It occurs independently of the specific arrangement of the series, and appears to be due in part to the intrinsic qualities of the colors. The factor of immediate contrast on this illusion was not measured.

SOME CHARACTERISTICS OF THE WORK-CURVE WITH SHORT WORKING UNITS

By L. T. Morgan, University of Bristol, England

Introduction

In 1916 F. M. Phillips described a research on the curve of work in the fundamentals of Arithmetic using 1-min. work-units with a large number of subjects.[1] The results show a surprising superiority for the first minute of the ten-minute sequence. The lowest point in the work-curves occurs in the second minute in eight of the twenty curves. In the remaining curves the lowest point is after the fifth minute. Chapman[2] had used 2-min. work-units with 46 men, the task being to add columns of ten one-place numbers for two 10-min. sequences on each of 5 days. The curve which he constructed from these 460 measures showed a surprising smoothness. Individual variations were largely eliminated and the common characteristics emphasized. The results in his case again showed high initial performance with an abrupt fall for the first four minutes; this fall gradually becoming less abrupt till after 10 minutes when the curve had become almost horizontal.

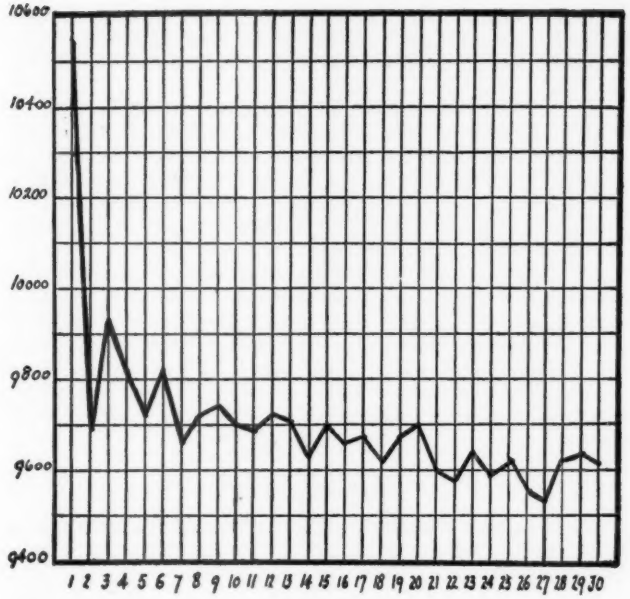
A common characteristic of these two researches is, then, surprising superiority of the initial performance. The question arises whether the division of the work sequence into units smaller than 2-min. would produce a work-curve with peculiarities not obvious from the larger divisions. G. E. Phillips,[3] who employed one-min. units, showed that some of the characteristics of both sets of the curves mentioned above appeared. The high initial performance still obtained; then there was a marked depression in the second minute of work; a rise during the third, and finally a gradual fall to the horizontal. Phillips used cancellation as a task with four subjects.

We must be quite clear, in work of this nature, as to what factors have been controlled by the conditions of experiment. Good technique is not content with a general exhortation to work as rapidly as possible. Incentives must be used to keep

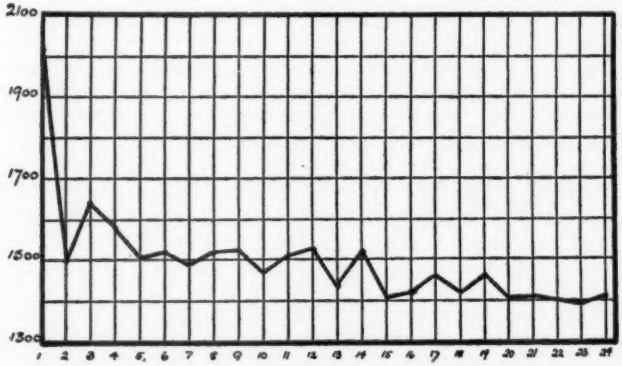
[1] F. M. Phillips, A comparison of the work done in successive minutes of a ten minute practice period in the fundamentals of arithmetic, *J. of Educ. Psych.*, 7, 1916, 271-277.

[2] J. C. Chapman, A study of initial spurt in the case of addition, *ibid.*, 6, 1915, 419-426.

[3] G. E. Phillips, Mental Fatigue, *Rec. of Educ. Soc.*, 1920, Teachers College, Sydney, 1920.



CURVE I
Curve of work for *Cancellation* in ½-min. periods



CURVE II
Curve of work for *Addition* in ½-min. periods

the endeavour as high and as constant as possible, especially in the case of experiments in the schools. We must be quite clear also as to the part played by practice, especially in experiments on fatigue.

G. E. Phillips used monetary rewards as incentives. The practice factor was controlled by a prolonged preliminary sequence. Not until the practice-curve had reached the horizontal, indicating that no further improvement at this stage was probable, was his experiment proper commenced. The readings through forty-five days which constituted his experiment show slight practice effects.

It is worthy of note, therefore, that, besides the general fall in the curve from a high initial performance to a horizontal which is a characteristic of all three researches, we find in the one-min. curve a minimal point at the second minute of work, but in the two-min. curve, no such peculiarity. It may be, as Phillips himself suggests, that the division of work into units larger than one-min. swamps the depression at the second minute.

The Curve of Work using Half-minute Working Units

In our own experiments an attempt was made to find whether these characteristics were obvious with a 12-min. work sequence divided into half-minute units.

A preliminary practice was first carried out. Cancellation of four letters from a sheet of capitals was given for 15 minutes each day, at the same hour, for 23 days. At the conclusion of this period the curves for all three *S*s had been horizontal for 6 days. Though slight variations were obvious from day to day, the main effects of practice had clearly past.

Incentives were used in the form of rewards for improvement. The first experiment consisted of a continuous addition test of 12-min. duration divided into ½-min. units, on 40 successive day, excepting Saturdays and Sundays.

The total scores by ½-min. for all *S*s are represented in the curves I and II above. The main characteristics are: (a) high initial output; (b) a minimum-point for the second half-minute; and (c) a gradual fall from the third half-minute to the horizontal.

It will be noticed that the minimum-point occurs at the second half-minute not at the second minute, which seems to suggest that it is the second point on the work curve, not essentially the work done during the second minute, which determines this point. This depression for the second unit-performance is clearly marked in all three individual curves.

The degree of reliability that attaches to the difference between two readings can be estimated by the formula (Whipple):

$$\text{P.E.} = 0.6745 \sqrt{\frac{\sigma_1^2 + \sigma_2^2 - 2r\,\sigma_1\,\sigma_2}{n}}$$

As the difference between the mean performances for the second and third points on the curve is 6.3 with P.E. 1.9, the result appears to be not a matter of chance or random error.

The same experiment was carried out using, first, simple addition with three *S*s, and secondly, dictation of numbers at a maximal speed from a Kraepelin sheet, with one *S* under similar conditions, after a preliminary practice period of 20 days. Both average curves, as well as all four individual curves, show the characteristic high initial performance, a minimum at the second ½-min., a subsequent rise, and a gradual fall. Again the evidence tends to show that it is the second work-unit not the second minute which shows the characteristic depression. The characteristic form appears to be independent of the task and common to all subjects. This latter result was arrived at also by G. E. Phillips with 1-min. work-units.

In the cancellation test 7% more work was done in the initial half-minute than in the average of the subsequent three ½-min. periods. For the addition test the percentage was found to be 28%, and for the third test 9%.

The Effect of Fatigue

Another interesting point of comparison between the curves of Chapman and G. E. Phillips is that the effect of fatigue seems to be confined to the first twelve minutes of work. Chapman does not quite reach the horizontal stage; but it is quite clear that, if the curve were prolonged in accordance with its general trend, it would achieve the horizontal at approximately 12 min. We have already pointed out that for these two curves entirely different work was involved.

Referring to our own curve for cancellation, it will be noticed that the fatigue-effect is confined to the first thirteen minutes—the curve becoming horizontal before the 26th half-minute. In the second experiment, using addition, the curve becomes horizontal just after the 12th min. (24th half-min.) of work.

There would appear, then, to be some additional evidence for G. E. Phillips' assertion that the unsatisfactory work-curves of the Kraepelin School, using 5-min. units, are due in part to the failure of recognising that the fatigue-effect is confined to the first three points on the curve.

We might further question whether the total depression from the initial point to this horizontal phase bears any relation to the nature of the task or to the individual. Lehmann[4] found in ergographic records that the depression bears a relation to the rate of working. Where the interpolated rests are short the depression is great; with rests of a certain length there is no depression. In other words, there is no fatigue-effect when there is no depression; the depression is greatest when the fatigue is greatest. Lehmann maintained that the depression is inversely proportional to the length of the rest interpolated between each contraction.

G. E. Phillips found that, in cancellation, the depression varied in the case of his three individual curves. By examining the depression in three different tasks for one *S*, he concludes that there is a probability that the depression is dependent on the nature of the fatiguing work.

If we standardise, for comparison, the depression of a number of points on the curve as a percentage of initial performance, thus, $\% = \frac{d \times 100}{1}$ (where d = depression and 1 = initial performance) we get the following table.

TABLE I

Cancellation

S	20th	22nd	24th	26th	28th	Mean	P.E.$_M$
1	8.8	9.0	7.5	9.4	9.0	8.8%	.21
2	8.1	7.5	6.8	10.6	9.7	8.6%	.42
3	7.5	10.7	10.1	8.4	8.4	13.0%	1.25

Addition

S	18th	20th	22nd	24th	Mean	P.E.$_M$
1	28.9	25.3	24.1	28.9	26.8%	.72
2	24.4	28.6	36.5	30.0	30.1%	1.47
3	35.7	39.3	34.0	32.1	35.3%	.89

Dictation of Numbers

S	20th	22nd	24th	26th	28th	Mean	P.E.$_M$
1	11.9	13.5	13.5	14.4	14.1	13.5%	.33

The depression of the horizontal phase below the initial is certainly not constant for all our *S*s. Both in addition and in cancellation there is a clear difference between the percentage depressions. Our *S*s are not equally fatigued by working at

[4] A. Lehmann, *Grundzüge der Psychophysiologie*, 1917.

maximal pressure for the same time at the same task. These figures, then, indicate the *fatiguability* of the *S*s for specific tasks.

What is the relation between this fatiguability and the ability of the worker as represented by his initial performance?

TABLE II

	Cancellation		Addition	
S	Initial Perf.	Fatiguability	Initial Perf.	Fatiguability
1	400	8.8	83	26.8
2	320	8.6	63	30.1
3	335	13.0	56	35.3

In these three cases (see Table II) ability in a task seems to be inversely related to the fatiguability of the individual for that task. But no generalisation is possible of course from only three *S*s.

Does the depression depend on the nature of the task?

TABLE III

S	Cancellation	Addition	Dictation (Numbers
1	8.8%	26.8%	13.5%
2	8.6%	30.1%	
3	13.0%	35.3%	

On reference to Table III it will be evident that the depression for addition is over three times the depression for cancellation for the same individual.

This cannot be explained entirely in terms of random variation. Table I implies that the standard deviation and the P.E. for the sequence of measures are indicative of reliable data. Random error is certainly not significant enough to account for the difference between the readings for the three tasks. It appears, then, that the depression varies with the individual and with the task which produces the fatigue.

THE PLACE OF OCULAR MOVEMENTS IN STROBOSCOPIC PERCEPTION

By GLENN D. HIGGINSON, University of Illinois

It has long been known that the apprehension of movement in the visual field may, under certain conditions, rest upon slight involuntary shifts in ocular fixation. Thus the apparent movement of objects experienced after bodily rotation has been attributed, at least in part, to nystagmus; for, with the cessation of the eye-movements, the world no longer spins. Again, purely mechanical manipulation of the eyeball with the finger will give rise to the visual perception of movement. Pressure in this case seems to change the retinal pattern in such a way that certain objects are actually seen as moving although the remainder of the visual field may be at rest. We also know that observers report, in connection with the usual laboratory experiments upon the projection of the after-image, that the after-image moves about in various directions as a result of involuntary eye-movements. And, finally, various illusions of motion are commonly (though not always) referred to slight and unobserved shifts in fixation; *e.g.* autokinetic movements observable with a dim light in a dark field; a type of movement frequently reported in an earlier study of ours,[1] where we used a dim fixation-light in total darkness.

It appears, then, that ocular shifts variously produce the perception of movement. Is this *generally* true? Or are movements of the eyes but one of many ways in which this perception may be induced? What shall we say of the perception of movement under discrete retinal impressions where points on the retina are successively stimulated? Can we here too invoke eye-movements as an explanation? The purpose of this paper is to point out certain facts which were observed in connection with a study of such apparent movement and to show that the *assumption* of slight ocular shifts as a condition of the apprehension of apparent motion introduces a common element of unity by means of which a fairly large number of these facts can be bound together. We do not offer these facts as *proof* that eye-movement is a general condition of these perceptions; but we merely suggest that such an assumption most adequately explains the observations in question.

(1) *Observers report ocular movements.* The clearest evidence of eye-movements under the method of successive stimulation

[1] This JOURNAL, 37, 1926, 63-115.

such as we used comes from the actual experiences of our observers.[2] *Os* reported clear kinaesthesis from the eyes with the meaning that there was a change in fixation when the lines appeared. They also reported that the eyes seemed to follow the lines up and down, or that the eyes jerked up or down between the appearance of the first and the second object. Others interpreted these experiences by the conjecture that while they saw clear movement it must have been given kinaesthetically.

(2) *Presence of intermediate members.* Every one of a score or more of our *Os* reported, at times, that there were three, four or five successively seen lines in the field or that one line in moving stood out sharply as many times. That is to say, all reported the existence of various members intermediate to the two terminal positions. This sort of report upon the presence of secondary lines is not limited to our own experiments. Some of Dimmick's *Os*[3] also apprehended them; and they are diagrammatically represented in Dimmick's later study upon apparent and real movement.[4] In our study these figures were, as a rule, not simultaneously apprehended. There was an observable sequence, the particular direction depending upon the order of appearance of the stimulus objects. Moreover, these intermediates were generally less intense and not so clean-cut as the end-members. Often they were decidedly fragmentary or broken into halves or thirds. When colored lines were employed as stimulus-objects the saturation and the brightness of the lines tended to be less than in the end-positions.

(3) *Movement of negatively colored objects.* Certain *Os* reported that a bluish-green line or a blue line of low chroma moved across the field. These reports came when we were using the slitted cards with standard mazda bulbs. Under these conditions the lines were actually yellowish in hue. Here, as it seems, is a clear indication of a change in fixation with the appearance of negatively colored after-images. That such negative effects may occur under short exposures we know from Mach's work upon the simultaneous apprehension of colored objects under short periods of illumination.[5]

(4) *Movement beyond the second member.* From time to time *Os* observed one line move to and beyond the position of the second figure and then immediately back to it. They saw a weak line where no line could possibly have been, *i.e.* below or

[2]For apparatus, method and observers the reader is referred to the article cited, pp. 76-79.

[3]F. L. Dimmick, An experimental study of visual movement and the phi-phenomenon, this JOURNAL, 31, 1920, 317-332.

[4]F. L. Dimmick & C. H. Seahill, Visual perception of movement, *ibid.*, 36, 1925, 416.

[5]E. Mach, *Beiträge zur Analyse der Empfindungen*, 1906, 205-6.

above the position of the second nember, depending upon the order of sequence. Most *O*s saw the first line actually overshoot the final position and subsequently withdraw to it before disappearing. Others interpreted the experience differently. It meant that the one line by assuming the position of another forced the latter for a moment from its position. Regardless of this difference in interpretative report, all saw a weaker line that appeared momentarily beyond the position of the second member before moving back and disappearing.

(5) *Dual movement.* The assumption of eye-movements seems also to cover the facts of dual movement. Where the intervals were very short the *O*s sometimes reported that the two lines appeared and moved together, as if cut from a single piece, in one or two directions. If the sequence was horizontal-oblique the movement was upward for a few degrees and then down; the reverse order of presentation gave movements in the opposite directions. The upper and lower limits of each of these movements might be defined by lines which formed two small angular fields filled with light over which the movements occurred. We know that where one line is added to another, without interval, there is movement in the direction of the added member and that, when one member is subtracted from the total figure, movement is in the direction of the remaining line. If we bring two lines almost simultaneously into the field, but with a slight temporal interval, one sees first a movement of both lines in the order of sequence. But if the first disappears a trifle before the second, it is quite possible that the eyes shift again in the direction of this change in the visual field due to the disappearing line and thus condition movement in that direction. We do know that the eyes tend to shift toward any change going on in the visual field. May we not assume, then, that the eyes shift downward and then back under the sequence of appearance and disappearance and that along with this shift go the after-images of the lines in various positions? The apprehension of these lines together with ocular kinaesthesis would then produce the apparent dual movement.

(6) *Spatially dislocated lines.* Another group of facts pointing towards the occurrence of eye-movements at the time of apprehended motion has to do with spatially dislocated members. The position of each line with respect to the other varies from time to time in a puzzling way. The oblique may be seen as a vertical, as a horizontal, or as an oblique, but so reversed in position that it now approaches the right end of the horizontal instead of the left. In addition to these changes in the position of the lines, there are slight variations in the position of the oblique varying through 20-75 degrees. Furthermore, the horizontal may be seen as an oblique. This upward shift of it oc-

curred especially when the oblique was seen around a vertical position. With the parallel-horizontal arrangement of the lines, one member might actually be seen moving as an oblique. The clearest evidence in the general connection of the dislocation of the lines comes, however, from the way in which the true oblique might move to the horizontal or the horizontal might move to the oblique. In either case there was frequently no clean-cut turning movement as around a pivot. The lines seemed to move *directly* up and down. That is to say, the oblique moved as an *oblique* so that it actually bisected the horizontal.

(7) *Momentary disappearance of the first member during movement of second.* When one member was added to the other, *O* occasionally reported that during movement the first line momentarily disappeared only to reappear a fraction of a second later and to stand out clearly with the second member, forming in this manner either an angle or two parallel lines. This same experience has been reported in connection with a problem upon which we are now engaged, *viz.*, the apprehension of visual movement with short delays but without temporal intervals. Our more naïve *O*s sometimes report that the earlier of the two lines, to which the other is added, disappears for a second while the second is moving to its final position. Here again is a clear indication of change in fixation during movement.

(8) *Movement of a single line.* At times a line was actually seen to be *moving* when it appeared in the field; *i.e.* movement was simultaneous with appearance. We find a similar sort of report in Dimnick's study (pp. 325-328). Good visual movement of one member may be followed by the perception of the second member in a stationary position. Movement, then, may actually be apprehended before the second member appears. It seems to be independent of the second member except that it is in the direction of the subsequent position of that subject. It is as if the organism anticipated the order of phenomenal succession and apprehended the first as moving even before the other appeared.

(9) *Phenomenal differences in intensity, brightness and extent of the stimulus objects.* When the two members were given together by turning on both lights within the apparatus and a detailed description called for, *O*s reported that both objects were equal in intensity, brightness and extent. When the lines were successively presented, however, the one member might be apprehended as much more intense or as brighter than the other. *O*s frequently commented upon the striking differences in intensity, brightness and extent of the members under the movement experience. Assuming always a constant fixation, it is extremely difficult to understand why the objects should not manifest a fairly constant degree of similarity even though ex-

position-times (equal for both) were short. But if we do not assume a constant fixation the matter is made clearer. Then these differences lend themselves to an explanation in terms of rapid movements of the eyes; because what is seen at the beginning or during a flickering glance is unlike that observed when the eye is fixed.

These nine groups of facts embrace a large range of observations. We have not attempted any quantitative survey of the total number of reports. Our primary interest has been to call attention to this striking variety of facts and to raise the question whether they may not be brought together and made more intelligible through the *assumption* of eye-movements.

It has been asserted that eye-movements cannot be significant for the visual apprehension of movement because the temporal intervals between the exposition of the successive members are smaller than the time necessary for the movements of the eyes to occur. This argument, we must insist, is wholly fallacious. One cannot argue directly from the physical order to the phenomenal. The rise and the temporal course of the neural excitation is not identical with that of the stimulus. Because one member follows another with a certain fixed temporal interval does not imply that the second will be seen at the precise moment of its presentation. Members of the *Gestalt* school have tried, as we know, to set up a precise correspondence between the physical and the phenomenal—a sort of physico-phenomenal constancy; but we have but to turn to a major investigation by Korte (a student of Koffka's) to see this 'constancy' break down. Korte shows that, although the physical sequence may be of one sort, the phenomenal movement may be in the opposed direction.[6] Bethe had already demonstrated the same fact. Thus when a series of adjacently placed lights was simultaneously lighted the observer obtained a clear sequence along the series, the direction of the sequence depending not upon the order of the physical lighting but upon other ("subjective") factors.[7]

Conclusion. It is commonly agreed that certain visual perceptions of movement of the clearest sort are conditioned by movements of the eyes. Can we say as much for the perception of apparent movement under discrete retinal stimulation? At least we have evidence that eye-movements do frequently occur under such stimulation. This evidence appears (1) in the direct reports of the *O*s upon eye-movements; (2) in the presence of various intermediate or additional members; (3) in the movement of negatively colored lines; (4) in the actual movement of a line before the second member is seen; (5) in simultaneous movement of two lines in the same direction; (6) in the spatial dislocation of the stimulus object; (7) in the momentary disappearance of the first member while movement is in progress; (8) in the movement of a line beyond the position of the second

[6] A. Korte, Kinematoskopische Untersuchungen, *Zeits. f. Psych.*, 72, 1915, 193-296.

[7] A. Bethe, Beobachtungen über die persönliche Differenz an einem und beiden Augen, *Arch. f. d. ges. Physiol.*, 121, 1908, 1-12.

member; and (9) in striking phenomenal differences in the intensity, brightness and extent of the stimulus objects. Now these facts certainly point to eye-movement as a common condition; and, on the whole, they seem best to be explained from this direction. While we cannot assert that eye-movements chiefly determine the apprehension of movement under successive stimulation, we are sure that they do frequently occur at the time movement is seen. It appears not at all unlikely, then, that they serve as one of the conditions upon which perceptions of this general type rest. In view both of the factual and the theoretical importance of ocular movements in these stroboscopic perceptions it is obvious that the movements themselves must be very much better controlled and recorded than they have thus far been. Upon the experimental means to this end we are now engaged.

REACTION TIME TO PHOTOMETRICALLY EQUAL CHROMATIC STIMULI

By Joseph L. Holmes, Columbia University

Is the simple reaction-time to light conditioned only by the intensity of the stimulus or is it also influenced by wave-length? That the latter is true is implied by Henmon[1] who measured, by the method of discriminative reactions, the time for the perception of difference between pairs of colors. His experiments were limited in the main to the red end of the spectrum; though other pairs of colors, as well as black and white, were also used. The stimuli were derived from colored cards. No attempt was made to equate for intensity.

The following table gives a summary of the results from one *S*:

STIMULI DISCRIMINATED	REACTION TIME
Black and White	196.6
Red and Green	203.2
Blue and Yellow	199.8
Red and Blue	212.1
Green and Yellow	211.7
Red and Yellow	217.7
Red and Orange	245.2
Red and Orange mixed with 25% Red	253.3
Red and Orange mixed with 50% Red	261.2
Red and Orange mixed with 75% Red	269.5

In discussing these results Henmon says, "It is hardly necessary to state that the colors were in each case rich, saturated colors. There is, however, the normal difference in brightness, and as in the previous experiments it is the opinion of the writer that the differences in time of perception are due in some degree to the differences in brightness in the case of red and blue. Of the two variables present in the stimuli the color tone should have given, if anything, a shorter time,[2] and therefore the reasonable assumption is that the brightness causes the differences."[3]

In the present experiment the simple reaction-time method was employed and the stimuli were photometrically equated red, green, yellow, blue and white lights. The time of reaction was measured with a Hipp chronoscope in which a weight was substituted for the springs to balance the armature, the usual precautions being taken to insure accuracy by frequent tests with a gravity chronometer.

[1] V. A. C. Henmon, The time of perception as a measure of differences in sensations, *Arch. of Phil., Psych., and Scient. Meth.*, 1906, 1-75.

[2] By "shorter time" Henmon means that the reaction time, *i.e.* the discrimination time, should have been shorter.

[3] *Op. cit.*, 9.

S was concealed from *E* and the apparatus by a screen extending from the floor to a point well above his head. The table and screen were illuminated by a 50-watt daylight lamp at the back and to the right of the *S*'s head thus preventing dark adaptation. At the edge of the table was placed the head-rest of an opthalmometer which secured a constant distance (58 cm.) between *S*'s eyes and the stimulus and thus a constant visual angle. The stimulus was in the median line and on a level with the eyes. At *S*'s right, on the table, was placed an ordinary telegrapher's key with which he reacted by an upward flexion of the wrist.

Extending at right angles from the screen behind which *S* was seated, was a track 30 cm. wide and 407 cm. in length. On the track was a box 90 cm. high which moved on wheels. On this box at its end farthest from the screen was placed the mechanism which exposed the lights. This included two electro-magnets. From the armature of the magnet extended an arm about 3 in. in length which was attached to the back of a front silvered mirror hinged at the bottom. By allowing the current to pass through the magnets the angle of reflection of the mirror could thus be changed. The source of light was a 25-watt tungsten daylight-lamp in an optical box placed 40 cm. in front and below the level of the mirror. On the screen and on the level with the mirror and *S*'s eyes was a hole 3 cm. in diam. covered with finely sanded glass.

To expose the stimulus, then, a switch was closed, allowing the current (kept at 8 volts) to pass through the magnets moving the armature, which in turn deflected the mirror and so reflected the light from the optical box into the hole in the screen. Beneath the arm which connected the armature with the mirror was a small rod bent downward at each end to dip into mercury cups and supported at the middle by a pin passing through it to uprights, thus allowing it to oscillate. When the arm moved backward, deflecting the mirror, it tipped this rod, raising it so as to break the connection in one mercury cup, and thus starting the chronoscope, and immediately after making it in the other. The circuit through the lower coils of the chronoscope was broken by *S*'s key.

Between the source of light in the optical box and the mirror were placed gelatine filters furnished by the Research Laboratory of the Eastman Kodak Company. The light transmitted by them was as nearly monochromatic as is usually obtained by gelatine filters. They were equated with a standard flicker photometer at the Electrical Testing Laboratories, New York City, the source of light used in the photometer having been matched with the light of the lamp used as a source during the experiment. Then the relative distances at which the lights were photometrically equal were obtained by varying the distance of the mirror from the screen, these distances being proportional to the readings of the photometer-scale.

When photometrically equated the red, green and blue were rich, saturated colors, only the yellow being affected in color tone (said by the *S*s to be "gold color"). Described in wave-length the colored filters were as follows:

Green	560-510
Blue	510-360
Red	700-650
Yellow	700-500

E was seated at a table before and to the side of the screen with the chronoscope, a voltmeter and a switch for the magnet circuit before him. When this switch was opened the mirror tipped forward again, causing the stimulus to disappear. The duration of the stimulus was for the time of the reaction, the switch being opened as soon as the *S* had reacted. The armature of the exposure-apparatus was well padded and the other parts lubricated to prevent any distracting sound. The chronoscope was stopped only for rewinding.

The current used was that of the university lighting system reduced by rheostats to 4 volts for the chronoscope circuits and 8 volts for the exposure-apparatus. In the lamp of the optical box 120 volts were used with a voltmeter to detect any appreciable change. There was a constant fluctuation of two volts (119-121). Only once during the experiments was this exceeded, and then observations were discontinued until normal conditions prevailed.

The following directions were given to each *S*:

"The subject will seat himself facing the table with both arms resting upon it, the right hand at the key. He will look at the screen before him, focusing upon the hole where the stimulus appears. The experimenter will start the clock and say 'Ready.' The subject will then press the key with the first two fingers and get ready to react, giving his attention to the stimulus (the reactions are to be sensory). After the appropriate interval the stimulus will be exposed and the subject will react by releasing the key with an upward motion of the wrist."

The interval between the "ready" signal and the exposure of the stimulus varied from 2 to 4 sec.

Two *S*s (G and N) were employed, both thoroughly practiced, having previously served in extensive experiments upon reaction-time. G gave two sets of reactions to the five stimulus lights (including white.) In both sets 10 reactions to each stimulus made a series, the order being varied to counterbalance the effects of fatigue and practice. Each series began with a new stimulus, the first stimulus of the second series being the second of the first and so on until each stimulus had occupied each position twice. N completed one set.

The following tables give the average reaction times to each light, their mean variations and probable errors, and the differences with their probable errors.

TABLE I, SUBJECT G

Set i

	WHITE	GREEN	YELLOW	BLUE	RED
Av.	167.89	169.16	169.74	170.19	170.59
M. V.	2.91	4.27	6.39	2.92	6.53
P. E.	.77	1.14	1.70	.77	1.74
Differences		1.27	.58	.45	.40
P. E.		1.54	.82	1.06	1.77

Set ii

	YELLOW	WHITE	RED	GREEN	BLUE
Av.	177.93	178.43	178.98	179.48	180.0
M. V.	4.74	5.09	4.96	5.43	5.18
P. E.	1.26	1.35	1.32	1.44	1.38
Differences		.50	.55	.50	.52
P. E.		1.21	1.20	1.01	1.39

TABLE II, SUBJECT N

	BLUE	YELLOW	GREEN	WHITE	RED
Av.	171.51	172.29	172.84	173.49	176.25
M. V.	9.07	8.35	9.20	7.88	9.85
P. E.	2.42	2.22	2.45	2.10	2.62
Differences		.78	.55	.65	2.76
P. E.		1.25	1.77	1.87	.92

Inspection of the tables shows that the differences in time are small and unreliable. In each set the order for the five lights is different, being as follows;

S		GREEN	BLUE	WHITE	RED	YELLOW
G	Set i	2	4	1	5	3
	Set ii	4	5	2	3	1
N		3	1	4	5	2

Individual differences in the perception of brightness, as well as differences due to the action-time of the retina, would seem—if they exist at all—to be obscured by errors of observation. The results seem to justify the conclusion that the time of reaction is not a function of wave-length.

A LABORATORY FOR THE STUDY OF CONDITIONED MOTOR REFLEXES

By H. S. LIDDELL,[1] Physiological Field Station, Cornell University

It is unfortunate, in view of the theoretical importance of data on the conditioned reflex, that so little information is available concerning the construction of a suitable laboratory and the technique of conducting these experiments. Practically all the information has been published in Russian and is inaccessible to American investigators. With this in mind it was thought that a description of our laboratory which was established (in the course of an investigation on the effect of thyroidectomy on the higher nervous functions of the sheep and goat) for the study of conditioned motor reflexes, might prove helpful to those contemplating similar experiments.

The animal room, shown in Fig. 1, is partially sound proofed by a layer of heavy felt, a 4 in. air space, and an inner lining of celotex. Between the animal room and the apparatus room (Fig. 2) is a window fitted with 4 panes of glass, with air spaces between, through which the experimenter can observe the animal's responses. An animal room completely sound and vibration proof is, of course, the ideal but as our laboratory is situated in an orchard away from the road and some distance from the city the precautions described have proved adequate. It is also desirable, as Pavlov has pointed out, to maintain a constant room temperature and to employ artificial illumination of fixed intensity. Moreover, the experimenter should be invisible to the animal during the experiments. This requirement can be met by working behind a screen or, better still, in another room. The above precautions are necessary to shield the experimental animal from stimulation by fortuitous environmental agents such as sudden noises, cues obtained by the animal from watching the experimenter's movements, etc. In addition to a suitable room for the animal, apparatus is necessary for the registration of the stimuli presented and of the responses obtained. This apparatus is shown in Figs. 2 and 3.

A brief recital of the procedure employed in establishing a conditioned motor reflex to tactile stimulation will be the simplest way of describing our equipment. The animal is taught to remain quietly in the straps suspended from a beam above the table. The wool is shaved from the skin of the left foreleg and a leather bracelet wrapped with brass wire (Fig. 1, A) is attached to the limb. This bracelet is connected with the secondary coil of an inductorium in the adjoining room. The primary coil is in circuit with a key and electromagnetic signal so that the

[1]National Research Fellow in the Biological Sciences.

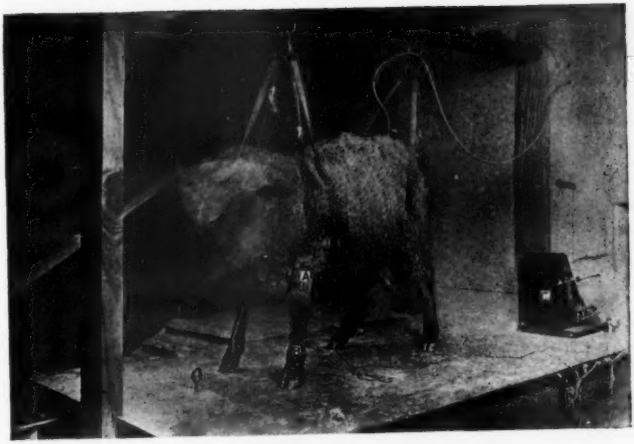


FIGURE 1

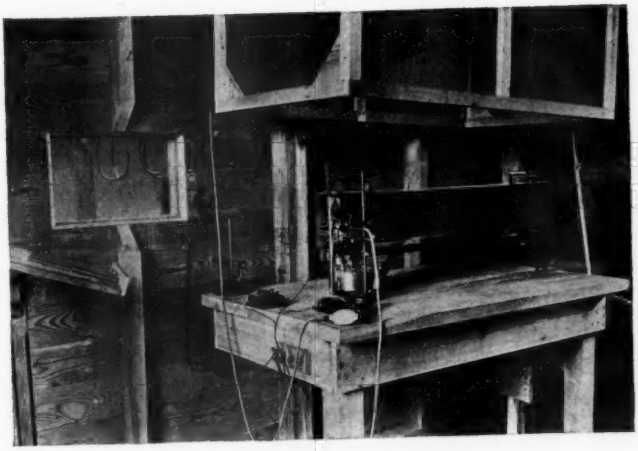


FIGURE 2

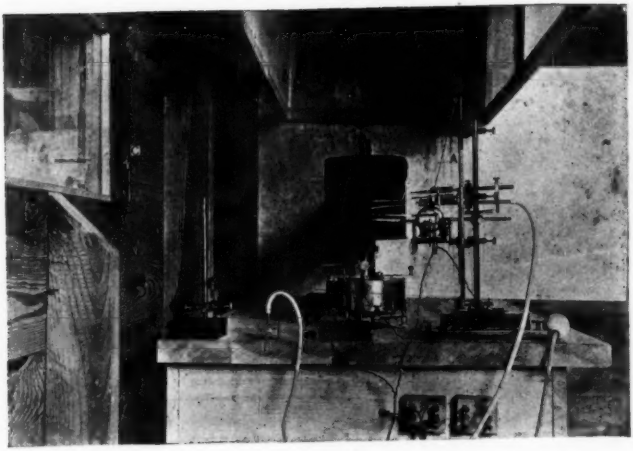


FIGURE 3

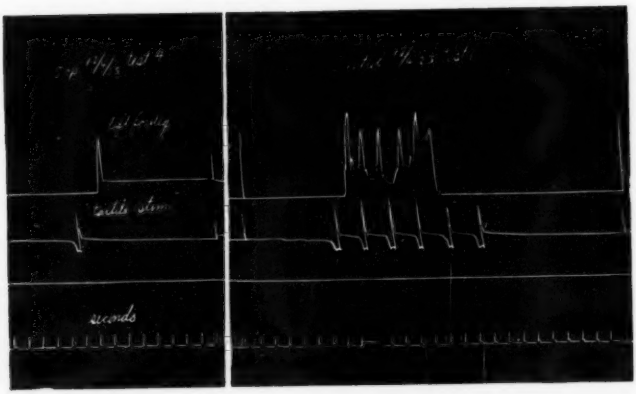


FIGURE 4

application of the faradic current is recorded on the kymograph shown in Figs. 2 and 3. When the electric shock is applied to the skin of the animal's leg a defensive movement results. A thread attached to the leg by a light leather strap (Fig. 1, B) transmits the movement to a reducing lever (shown best in Fig. 3, A) which records it on the smoked paper. This unconditioned response of the leg to the noxious stimulus depends upon permanent connections within the animal's nervous system. To build up a conditioned motor reflex to tactile stimulation, involving the formation of temporary connections in the nervous system, a small skin area is shaved and a spot tatooed. The tactile stimulus is delivered by means of a tambour (for purposes of description shown in Fig. 3, B). To the rubber membrane of this tambour is cemented a wooden block bearing the stimulating agent, which consists of 5 sharp points—pins, coarse bristles, or any other form of tactile stimulus may be used. This tambour is actuated by a rubber bulb and a side tube leading to a recording tambour makes possible the registration of the stimuli as they are presented. The perforated brass disc of the stimulating tambour, through the aperture of which the pins pass in descending to press upon the skin, is fastened to the skin with a cement composed of resin, beeswax, and ferric oxide. The stimuli are delivered once every two seconds and precede the application of the electric shock from 2 to 12 sec. The conditioned stimulus is also at times applied coincidently with the shock. The interval between combinations of conditioned and unconditioned stimuli is varied from 30 sec. to 5 min., for only in this manner may the development of a conditioned reflex to a definite time interval be avoided.

Fig. 4 shows two records of conditioned reflexes of the type just described. The top line of the tracing shows the leg movement resulting from the stimulation of a spot on the skin, traced by the recording tambour on the second line from the top where the drop of the horizontal line denotes the moment of tactile stimulation. The third line from the top marks the application of the induction current to the skin but in the tracing shown no shock was delivered. At the bottom of the tracing time is recorded in seconds.

The metronome provides a convenient conditioned stimulus. Its operation when used in the conditioned reflex experiments will be clear from an inspection of Figs. 1-3. The metronome is placed behind the animal (Fig. 1, C) and can be started and stopped by the experimenter from the adjoining room by means of a rod passing through the wall, bearing at one end a short bar and at the other end (Fig. 2, C; and Fig. 3, C) the knife of a single pole switch. When the switch is closed the bar releases the metronome pendulum and at the same time an electric circuit is closed through a signal magnet which traces the metronome beats on the smoked paper of the kymograph.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF NEBRASKA

Communicated by WINIFRED F. HYDE

I. SPATIAL SYMBOLS IN THE APPREHENSION OF TIME

By J. P. GUILFORD, Cornell University

The following Study was an attempt to determine the extent to which time tends to be apprehended in spatial terms, and whether there are any space symbols for time which occur more or less universally. The results bear upon the problem of imagery in thought as well as upon the perception of space and time.

The method is introspective, with the exception of two parts of the Study in which motor attitudes of the *S*s were observed. The *S*s were an unselected group of students in general psychology. All of them were somewhat acquainted with introspection, since the experiment was given near the end of their course.

The investigation was inspired by the observation that, in the writer's own experience, time takes rather definite and vivid spatial attributes. For example, the author finds his past projected to the rear and downward, with the future extending to the front and upward. From various other observations and the reports of temporal experiences of others, the following questionnaire was developed, along with a list of instructions for applying it to the *S*s:[1]

INSTRUCTIONS AND QUESTIONS

A. See that the student is sitting up facing the front, head up, eyes to the front, yet not in a rigid position. Read these instructions and watch the student for any significant motor responses. Does he look up or down?

(1) Think of some event or circumstance connected with your graduation from high school.

(2) Recall your earliest school experience, your first day at school, if you can.

B. Have the student write answers to the following questions:

(1) What spatial form does your past take, *i.e.* how is it presented to your mind or imagination? Describe it as accurately as you can.

(2) What is the earliest memory you can recall? How old were you at the time? Give age in years and months as nearly as you can.

C. Read the following and watch for significant motor reactions:

(1) Imagine yourself 5 years from now. 20 years from now.

(2) Imagine yourself at the age of 70.

D. Have the student write answers to the following questions:

(1) What spatial form does your future take, *i.e.* how is it presented to your mind or imagination? Describe it as you did your past.

(2) If you were to represent your past, present, and future by a drawing, what sort of a figure would you make? Draw it.

(3) What form (or qualities) does all historical time, past, present, and future, take for you? Represent by means of a drawing if it takes any definite spatial form.

[1]Acknowledgment is made to Professor H. B. Alexander who suggested the problem and gave helpful suggestions for the questionnaire.

(4) What does a year mean in your time experience? a month? a day (24 hours)? day and night?

(5) How do you picture the day upon rising in the morning? upon retiring at night?

(6) How do you picture the night upon waking in the morning? upon retiring at night?

(7) What significance, spatial or temporal, attaches itself to New Year's? to Thanksgiving? to the 4th of July?

(8) How do you picture the passing of time at the present moment?

(9) How do you picture 3 p. m. in your imagination? 3 a. m.? 12 noon? 12 midnight?

(10) What would be the effect of turning the clock ahead one hour on your time experience?

Note: Do not in any way suggest to the student what you think is the normal time conception.

If questions (4), (7), or (9) in D are especially difficult, suggest that the student get a 'snapshot' of his consciousness as the words 'New Years,' '3 p. m.,' etc., are spoken. Encourage graphic representation in the student's answer to any of the questions.

Some of the questions have no direct bearing upon the present purposes of the experiment, but it was thought that the opportunity should be taken to ask for other items of interest relating to time experience. This report mentions the results of only those parts which are most significant to our problem.

When the instructions under *A* and *C* were read, the *S*s were watched in order to observe any significant motor responses or attitudes. This was done to test a supposition that, if it were the common response to think of one's past as back and downward, *S* in attempting to recall some past event would tend to look down, and, if the future for most individuals is projected to the front and upward, there would be the opposite tendency to look up or forward. Two difficulties presented themselves in getting numerical data on these general motor tendencies of our *S*s. In the first place, the questionnaire was read to groups of 10 to 12 *S*s and it was impossible for *E* to get an accurate count of the number looking up or down. Secondly, it was difficult to decide in some cases, due to movements on the part of *S*, whether to class him with the 'ups' or 'downs.' It was, however, the consensus of opinion of those who gave the experiment, that, while there is a general tendency for *S* to look down when thinking, either of the past, of the present, or of the future, the tendency is much greater when thinking of the past than when thinking of the future. In the latter case there is a slight tendency to look upward.

An important part of the Study was question *D* (2), in which graphic representations of one's personal past, present, and future are called for. Eighty *S*s, or 91% of those who were questioned, were able to comply with the instruction. The responses were extremely varied and classification of the figures is rather difficult, but, with the exception of one, which represents time as an expanding circle, they can be roughly grouped into 5 types. The classification merely shows the direction of the curve. All horizontal movements, with one exception, were from left to right, a fact which is doubtless due to our conventional habits of writing. The vertical movements are significant. Forty-six *S*s, or 58% of the cases who reported spatial forms, represented the past below the present and the future above it. In other words, the curve has an upward slant; it ascends. The simplest figures of this type were curves beginning with a straight line which rises at an angle of 45°, representing the past, continues in a horizontal line, representing the present, and ends again with a 45° rise, representing the future. Twenty-six figures were as simple as this, with only minor variations. Other figures of the same type were more complex, such as one in which

the individual represented himself climbing an incline resembling a ladder, or another, which showed a wheel rapidly spinning up a hill. Four cases of the ascending type lay only in the vertical direction. The next most frequent type represents the past, present, and future on the same level; there were 20 cases, or 25%. Some of these figures were merely lines, straight, wavy, or broken. Others were three-dimensional figures, 3 of them with two cones whose bases are in common at the center (the present), and 5 with two cones or pyramids with their apices joining at the center to represent the present. Ten *Ss*, 12.5%, pictured the present at the crest of a wave; the curve was convex upward. Two *Ss* drew a curve which was convex downward. In only one case was there a descending curve, and then the drop was a vertical one.

Several difficulties present themselves in an interpretation of these results. We are not sure that the figures drawn by the *Ss* are necessary for the meaning of extended time or, granted the necessity, that we know that they are the *only* ways in which the *Ss* could represent their time experience; therefore we can not certainly call these forms habitual images, although they appear to belong in a class with number forms and the like. Nevertheless we may assume that the figures obtained at any one time are a fair sampling of the most suitable or common spatial forms. It may be objected, too, that the *Ss* were influenced by the conventional symbols which represent life as an uphill struggle or progress. While this may be true in part, it is also true that a negligibly small number of *Ss* used any such current figures; most of them drew rather abstract and simple curves. In any case we have here a sample of the time conceptions held by people at large. Again it must be noted that we are not sure that in 90% of our *Ss* spatial images are functional in their usual time experiences, for in this experiment some *Ss* had to be urged to find a spatial figure to represent time. For some the task was easy: a figure readily suggested itself; for others it was difficult. Whether this difference is due to lack of spatial imagery or of the power of introspection we are unable to say. Since 90% report spatial imagery, however, the chances are that it is present universally in some degree, and undoubtedly contributes to the meaning of time experience. The data therefore tend to show that every individual carries with him a spatial frame of reference in which events, past, present, and future, find a place, and that there is some uniformity in the general character of these spatial forms.

The answers to question (3), concerning the images of historical time, throw light upon the same problem. In this case all *Ss* reported imagery of some sort, but only 69, or 78%, drew abstract spatial figures. These 69 figures may be classified into three types. One type is a rising figure very similar to that representing personal time. Forty-three, or 62%, drew this type of figure. Horizontal figures were given by 21 *Ss* (31%), and 5 *Ss* (7%) made use of a circle or other closed figure. Other imagery reported was as follows: of historical events, 8 cases; of dates, 7 cases; a train, a stream, vast space, a container, one case each.

Answers to question (8), "How do you picture the passing of time at the present moment?" show some rather naïve introspections on the experience of the duration of time. Sixty-two of the cases reported some more or less definite images of the passing of time. Classification of these images was still more difficult, but for the sake of brevity of report they may be grouped, without doing violence to the original reports, as follows: a 'slipping away,' 7 cases; a line, 6 cases (3 a moving line); events, 5; rhythmic units, 5; a watch or clock (image of hands or of ticking), 5; a wheel, 4; a train, 4; simply 'a movement,' 4; an endless chain, 3; a flying arrow, 3; crest of a wave, 2; climbing a hill, 2; work being done, 2; a point, 2 (one a moving point); celestial movements, an hour-glass, a stream, a crawling snake, a crawling snail, 'hurling through space,' a 'zigzag,' one each.

What general conclusions can we draw as to the perception of passing time from this array of pictures of duration? It is true that our reports may be partly determined by the fact that the *Ss* were already acquainted with the elements which psychology has determined as aids in the perception of time. Nevertheless there were almost a negligible number of responses which gave simply the terms 'rhythm,' or 'events,' or 'bodily changes' as elements in the awareness of passing time.

Three general elements occur often in the list. The most frequent is that of movement, with at least 37 references either to some moving object or simply to movement in the abstract. A second element is that of rhythm: there were 17 references to time as being cut up into units of some sort. A third element, which is not quite so evident, is that referring to work being done, or of energy being expended. We may suppose that indirectly, by empathy, nearly all references to movement involve this element which depends upon kinesthesis for its basis.

Thus, so far as these introspections reveal, we can reduce the perception of time into the simple elements of kinesthetic sensations and images, accented by rhythms. Whereas the images secured in the reports upon long temporal extents tend to take the form of traversed space for the past, and traversible space for the future, the images given to represent the passing of time in the present are for the most part images of movement, or of the traversing of space. Thus it would seem proper to say that time is in a psychological sense 'traversed space.' This would not mean that space experience is more primary than time experience, for other data might show the converse also to be true, that the perception of space is dependent upon temporal images, or that space is 'lived time.' For example, the length of a line might be perceived because of an image of the eye-strains necessary to traverse it; the durative attribute of the strain would here be the most important aspect of it for the meaning of distance. Whether the 'lived time' during a bodily movement is more or less primary than the 'traversed space' we cannot say; both depend upon kinesthesis for their content and their meaning.

Conclusions

1. Ninety percent of our 88 *Ss* had spatial images of their past, present, and future of such definiteness that they could represent them by means of a drawing. Fifty-eight percent of these *Ss* drew a rising figure.

2. Eighty percent were able to draw spatial figures to represent historical time. Sixty-two percent of these figures were ascending, 31% were horizontal, and 7% some closed figure.

3. Seventy percent reported a definite image for the passing of time at the present moment; 60% of these picture it as movement of some sort, and the remainder experience it in rhythmic units, or as some work being done, or as energy being expended.

4. In more general terms, we may say that spatial images, which are probably often of the habitual type, carry the meaning of time and furnish a frame of reference for personal and historical time. The experience of passing time is often accompanied by images of movement, which again shows the importance of spatial images in the apprehension of time.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY

Communicated by JOHN P. NAFE

XXX. THE PSYCHOLOGY OF COUNTING

BY MILES A. TINKER

We originally began this investigation with the intention of studying the influence of peripheral vision in the visual process of reading.[1] As preliminary we started to repeat the work of Jastrow and Hall on counting visual stimuli.[2] Before we had progressed very far the problem shaped itself into a study of the conditions and process of counting. Our quantitative results are of little significance here and so we shall present merely the qualitative aspects of counting visual stimuli.

Dr. E. C. Sanford, Mr. C. E. Dickinson and Mr. M. A. Tinker, members of the graduate department of experimental psychology, acted as *Os* in the experiment.

In the first part of the experiment letters were printed (with a typewriter) 1 cm. apart in a horizontal row on a white background which was placed on the drum of a kymograph. The order of the letters was determined by chance. The *Os* viewed the letters through a slit 1 cm. high and of variable width cut in a gray card-board screen. In the preliminary work the width of the opening was varied from 1 to 5 cm., but in the later experiments the opening was 1 cm. wide. *O* held a card before the slit until the ready signal was given. The revolving drum gave the conditions of visual reading except that the eyes remained comparatively stationary and the letters or dots moved. In all except the preliminary work counting dots was used instead of naming letters. We had an even-spaced series, one dot at every centimeter, and 13 odd-spaced series where the distance between dots varied from 1 to 6 cm., the amount of space being decided by chance. This gave irregular grouping of the dots.

Procedure. Only a few observations were taken with the naming of letters. In the work with the even-spaced dots the number of units in the series was changed with each observation and the speed was varied from a very slow speed up to the maximum speed for the particular *O*. With the odd-spaced dots the series was changed with every observation but the speed was constant for any single sitting. However it was changed from slow to a faster speed in consecutive sittings. The maximum speeds were found at which the uniform and irregular series were counted correctly. At the end of every series *O* was questioned concerning the ease or difficulty he had experienced and also concerning his assurance that the number he had obtained was correct. At certain times *O* was also asked for a detailed introspection on an observation. The *Os* were thoroughly familiarized with the following directions:

"This is an experiment in counting. After the ready signal you are to remove the small card from the opening in the screen and as soon as the dots begin to appear you are to commence counting them out loud and continue to the end of the series. You must count each dot separately."

The maximum rate of counting uniform series of dots was found for 2 *Os* when the alphabet, French numerals and German numerals were used for the verbal response, and for one *O* when Chinese numerals were used.

[1] The experimentation of this study was done with Dr. E. C. Sanford at Clark University in 1923.

[2] Joseph Jastrow and G. S. Hall, Studies in Rhythm, *Mind*, 2, 1886, 55-62.

Results. Counting proves, on analysis, to be a reaction process. In some cases it is quite like the process in reaction-time experiments, in others a little more complex. The simplest case is reacting the same for each unit. This is like counting things that occur serially at a uniform rate such as counting the pulse-beat with a mechanical counter. Here the counter reacts to each stimulus as it occurs by a prearranged motor response or if he is using a single word he reacts by a prearranged speech movement. This form of counting is common among primitive peoples.[3] The rhythm in this simple form of counting is the same as in verbal response to a series of units. Verbal counting is practically the same as the simpler form except there is a different number name for each unit. It is an elaboration to the simple reaction and is like reacting in a specified way. The mental set is important. *O* counts as the stimuli appear, and every number as presented and responded to, induces an inhibition of itself.

In counting a uniform series *O* attempts to establish a rhythm as soon as the dots begin to appear. If he is able to do this in the first 5 or 10 units the counting goes on smoothly; if not, he is uncertain as to the correctness of the number obtained. The maximum rate of counting a regular series depends on *O*'s ability to establish this rhythm. Rhythm is obtained by grouping units in 2's, 3's, 5's and 4's. The 4 group is obtained by linking together two 2 groups. With medium rates rhythm is comparatively easy to obtain. With very slow speeds, however, it is harder and at maximum or above maximum rates it is still harder to get. Whenever rhythm is established assurance as to the correctness of the number obtained is high. On analysis it appears that high assurance consists in a complete motor response that begins and comes to summation before another response begins. This holds true for the counting of irregular series as well as for uniformly spaced single units. The reaction in counting a group must be completed before the response to the next group begins in order to have maximum assurance. Maximum rate for irregular spaced dots is 2 to 2.4 dots per sec.

When the alphabet or foreign language numerals were used in counting, the process was essentially the same as with other verbal counting except it was a little more complex due to less familiarity with the verbal symbols. Maximum rates for uniform series are: French, 1.7 and 4.6 dots per sec.; German, 1.8 and 1.9 dots per sec.; alphabet, 3.4 and 4.3 dots per sec.; Chinese, 1.7 dots per sec.; English, 3.6 and 4.8 dots per sec. For all *O*s the counting in English was fastest.

Simultaneity in counting is like reacting with a uniformly spaced ready signal and tends to a 'motor' form. The counter gives to every object a pulse of attention, making the corresponding word in the number series simultaneously with the pulse of attention.

When the dots to be counted occur serially but irregularly spaced the process is a little more complex and is more difficult than in the regular series and is analogous to reacting without a ready signal. It is more difficult to match stimulus with response than in a uniform series. Here the reaction cannot be simultaneous and tends to 'sensory' form. The maximum rate in this kind of counting depends on the effective memory span. When the rate for the instant is too fast for immediate connection of stimulus and speech reaction, the response is completed from the memory, and assurance is higher if there is sufficient space to allow the completion of the response for that group before the next dot or group of dots appear. Difficulty in counting irregularly spaced dots is influenced by the speed, grouping of the dots (number in group) and the space before and after any given group.

[3] L. L. Conant, *The Number Concept*, 1886; The Historical Development of Arithmetical Notation, *Ped. Sem.*, 2, 1892, 149-152.

Summary

(1) *Counting is in reality a reaction process.*

(2) It is much easier to match a uniform series of units with verbal responses than an irregular series.

(3) It is comparatively easy to count stimuli presented at a medium rate, more difficult at a very slow rate and very difficult at the maximum rate. The greatest difficulty comes when the counter is unable to establish any rhythm.

(4) Difficulty in counting accurately comes with lack of assurance that the matching is correct, and assurance consists of complete motor response.

(5) The original learning of the number sequence is like learning anything else, *e.g.* a series of nonsense syllables, but the learning is carried far beyond the point of recall. It is thoroughly mechanized and the expert counter is in effect a neuromuscular machine with qualities and functions strictly comparable with those of a mechanical counter of metal.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF ANTIOCH COLLEGE

I. The Apparent Warmth of Colors

By Meryl F. Mogensen and Horace B. English

There exists a widespread belief that colors have a fairly constant 'apparent' or 'psychological' warmth. In this experiment the question is raised whether this apparent warmth is able to affect one's judgment of objects tactually perceived. Using a slightly modified method of paired comparison, we presented 6 saturated colors from the Zimmermann series of colored papers. These were wrapped smoothly about two slide rheostats which were kept at a constant temperature of 42°C.

The more important part of the instructions was as follows: "This is an experiment to test the apparent influence of color upon the warmth of an object. The temperature of the colored objects will be changed from time to time. You are to test the warmth of the two colored objects simultaneously and give your judgment as to which seems the warmer in terms of the colors. . . . . You are to look at the cylinders while judging them." The cylinders were firmly grasped for the space of one second and the judgment given a second later to a ticking metronome. Between judgments the palms were laid upon a neutral grey surface kept at 36°C. Three judgments were made of each pair before change to another pair.

Twenty-five undergraduates, 18 men and 7 women, acted as *Ss*. The following table shows the number of times each color was judged warmer. The maximum possible is 30.

Table I

Number of Times Each Color was Judged Warmer

Subject	Green	Blue	Yellow	Orange	Purple	Red
A	21	10	12	18	15	14
B	24	16	12	18	5	15
C	15	18	15	7	14	21
D	18	21	15	5	17	14
*E	13	15	10	17	9	26
*F	19	16	19	13	10	13
G	14	15	13	20	12	16
*H	18	16	14	13	14	15
*I	16	19	13	20	8	14
*J	17	10	17	28	6	12
K	16	13	21	16	12	12
L	15	18	22	11	9	15
M	18	14	13	15	13	17
N	18	21	11	12	12	16
O	15	14	14	16	19	12
P	17	14	13	17	16	13
*Q	13	19	15	16	11	16
*R	17	20	19	14	7	13
S	14	16	12	20	11	17
T	15	13	20	9	19	14
U	16	20	13	12	9	20
V	17	11	18	16	14	14
W	12	19	10	19	15	15
X	19	16	20	14	7	14
Y	19	21	16	14	17	3
Total	416	405	377	380	301	371

*Indicates that S is a woman.

Individual preferences of a fairly constant sort appear; the exact significance of such preferences requires more extensive investigation. But it is clear that there are few significant and constant preferences common to the group. The probable errors of the differences are in most cases too large to warrant ascribing the difference to anything but chance. The outstanding exception is purple. The superior 'warmth' value ascribed to green and blue as compared with purple can scarcely be ascribed to chance; the difference in the number of 'warm' judgments in their favor is respectively 7, and 6.5 times the probable error of the differences. Less certain, but still hardly due to chance, is the greater 'warmth' of red, yellow and orange. The differences are 4.5, 4.3, and 4.1 times the probable error of the difference.

The curious thing about this result is that purple—especially the 'royal purple' here used—is generally considered a warm color while blue and green are cool colors. But purple is also less pleasant than other saturated colors and it is quite questionable whether we have not to do here with a judgment of pleasantness. The surprisingly low position of red is also probably not due to chance. Yet there is little question that red is to be placed among the 'warm' colors.

Although the data are slight, the conclusion seems warranted that the apparent warmth of colors is insufficiently intrinsic to enter into a total configuration or *Gestalt* in such a way as to modify the judgments of tactual warmth.

II. Reaction-Time Symptoms of Deception[1]

By Horace B. English

The chief finding reported by Marston under the above title was the exsitence of two types of 'liars:' good liars, whose word-association time is shortened and whose variability is less when they are attempting to deceive, and bad liars whose time and variability are both greater.[2] As well as we could from Marston's somewhat sketchy description of his methods, we repeated the first part of his experiments. Two lists of ordinary words were printed on appropriate small cards. Before turning up a card for reaction, the *S* was directed by *E* to read either the right or the left list, and to read from the top or from the bottom. To each word the subject was to react as quickly as possible with another word. Following a definite scheme, however, *S* at irregular intervals reversed *E*'s directions but endeavored to keep him in ignorance as to when the directions were followed and when they were reversed. To heighten the emotional tension, some stress was laid upon detection by means of the nature of the words pronounced as well as by the reaction-times. Time was taken for each list as as whole by means of a stop-watch.

Twenty unselected undergraduates took part in the experiment and coöperated heartily in the effort to 'beat the game.' Whether any or all suffered under the consciousness of deception may well be doubted.[3]

Of the twenty *S*s, 7 showed a significant increase in time when endeavoring to 'deceive,' while 9 showed a decrease. Six were significantly more variable in their 'deception' lists as measured by the mean variation, and 7 were less variable. But the *S*s who were faster were not necessarily the less variable, nor those slower the more variable. Marston's two criteria of the good and bad liars run criss-cross. In the following table, a +

[1]The writer wishes to acknowledge the assistance of Robert Edwards in acting as experimenter and in drawing up a preliminary report.

[2]W. Marston, Reaction-Time Symptoms of Deception, *J. Exper. Psychol.*, 3, 1920, 72-87.

[3]Compare E. R. Goldstein. The Consciousness of Deception, this Journal, 34, 1923, 562-581.

stands for increase in time or an increase in variability when 'lying.' Conversely, a − stands for shortened reaction time and less variability. o stands for a lack of statistically significant difference.

TABLE I
Subjects

Greater Mean	A	B	C	D	E	F	G	H	I	J
Reaction Time	+	−	+	−	−	o	+	o	o	−
Variation	+	−	−	+	−	+	−	+	+	o
Greater Mean	**K**	**L**	**M**	**N**	**O**	**P**	**Q**	**R**	**S**	**T**
Reaction Time	+	o	+	−	+	+	−	−	−	−
Variation	+	o	o	o	−	o	−	o	−	o

By Marston's two criteria, but 2 of our *Ss* are poor liars and 4 good liars. The rest belong to neither type.

The point raised by Goldstein seems to us, however, peculiarly relevant.[4] Does the experimental setting really involve the attitude of deception? Goldstein's results seem fairly conclusive that only irregularly is this the case. The question therefore remains for decision as to the factors which explain fairly consistent and significant differences between individuals in their response to the experimental setting. As no introspective reports were taken, we have no evidence. With all deference to our behaviorist brethren, it seems likely that a supplementary verbal response (introspective *Kundgabe*) would have thrown light on our results as it did on Goldstein's.

III. The Effect of Fatigue on Learning

By Margaret Shelley and Horace B. English

In 1920 Dr. J. J. B. Morgan described experiments to measure the effect of continuous mental work upon memorizing.[1] He used a modified form of the method of paired associates. Ten pairs of German-English equivalents were visually presented serially, 4 sec. to each pair. In recall, 2 sec. were allowed to the German word before the exposure of the English equivalent for a further 2 sec. Three such combined learning-recall periods were given each list. At their conclusion a new list was immediately begun. This was continued for 3 hrs.—850 pairs were employed.

We used Morgan's original word-list, and although our apparatus was manually rather than automatically controlled, the experimental setting seemed very similar. Four college students acted as *Ss*.

For scoring purposes the work-learning period was divided into 5 equal, consecutive sections of 36 min., during each of which the *Ss* worked on 17 lists or 170 pairs of words. The score is the average number of associates learned in each list of ten words.

TABLE I
Consecutive Periods of Work

Subject	1	2	3	4	5	Average
A	1.32	1.48	1.72	1.50	1.66	1.54
B	0.35	0.58	0.80	0.47	1.08	0.65
C	1.32	1.64	1.72	1.52	1.64	1.56
D	0.04	0.32	0.40	0.38	0.12	0.25
Average	0.75	1.00	1.16	0.96	1.12	

[4] *Cf.* G. J. Rich, Dr. Marston on Deception Types, this JOURNAL, 37, 1926, 307.

[1] J. B. Morgan, The Effect of Fatigue on Retention, *J. Exper. Psychol*, 3, 1920, 319-333. He carried out further experiments, as his title indicates, on retention. With these we do not deal.

As in Morgan's experiment, there is a tendency to show a spurt in the last time section. But where in his results the first section was otherwise the best, in ours it was the worst. Fatigue, uncomplicated by the end-spurt, might be looked for in the fourth period. Three *S*s do less well than their own average performance here, but the difference is a slight one, amounting to but 2.6, 2.7, and 1.3 percent in the 3 cases. The fourth *S* does better in this period than his own average. If the loss in this period is calculated from the *S*'s best score, a slightly better case can be made out but this difference is not great enough to be very important, even if statistically stable. These results are consistent with the earlier results of Oehrn, Woodworth, Vogt, and others cited by Morgan and somewhat at variance with his own. We must conclude that with untrained *S*s ability to learn paired associates does not act as a *test* either of fatiguability or of fatigue.

Indeed, is not the concept of fatigue an *ignis fatuus*? Muscular or nervous exhaustion are clearly defined conditions susceptible of measurement by suitably refined technique. But 'fatigue' is like charity in covering a multitude of sins, the chief of which is ignorance. One of the writers has recently suggested some of the discriminable factors commonly included under the term.[2] No doubt others can be found. The truth is that the term fatigue is popular rather than scientific. It is a symptom, not a disease syndrome, and has therefore about the same place in psychology as 'fever' has in medicine as a statement of a patient's ailment.

[2]H. F. Whiting and H. B. English, Fatigue Tests and Incentives, *J. Exper. Psychol.*, 8, 1925, 33 ff.

A REVIEW OF THE MARGERY CASE

By Walter Franklin Prince*

Margery the Medium. By J. Malcolm Bird. New York, Small, Maynard and Co., 1925, pp. 518. Referred to hereinafter as *M.M.*

Margery-Harvard-Veritas: A Study in Psychics. Anonymous; issued by M. W. Richardson, C. S. Hill, A. W. Martin, S. R. Harlow, J. DeWyckoff, and L. R. G. Crandon. Privately printed; copies can be obtained from Dr. L. R. G. Crandon, 366 Commonwealth Ave., Boston, Mass. 1925, pp. 109. Referred to hereinafter as *M.H.V.*

Articles in the *Journal of the American Society for Psychical Research*, vol. 19, 1925 (referred to hereinafter as *J.A.S.P.R.*), in *Scientific American*, 1924-25 (referred to hereinafter as *S.A.*), etc.

In May, 1923,[1] there blazed out the most brilliant star in the firmament of alleged physical mediumship that America has seen in fifty years, 'Margery,' the wife of Dr. L. R. G. Crandon, a Boston surgeon. At hundreds of sittings[2], it is claimed, 'ectoplasmic' limbs—extruded from her body and afterward reabsorbed—have performed various acts, such as touching persons seated nearby in the darkness, shoving, lifting and throwing objects, overturning a small table, ringing the bell in a box activated by a contact cover, producing phosphorescent lights, etc.[3] The establishment of these claims would have profound interest for science, since they imply the exercise of energy in a manner at present unknown to physics, and modifications of the human body revolutionary of present physiology. If it be a fact that the vocal and whispered utterances and the whistling sounds which are so frequent an accompaniment are 'independent,' that is, not produced by the vocal organs of any living person, and yet requiring the presence of a peculiarly-constituted person, the fact has deep significance for physics, psychology, and physiology. Also the appearance of live pigeons and flowers, either by special creation or by passage through matter, if factually established, would considerably enlarge the domain of either biology or physics.[4] Many other phenomena are alleged, and they all purport to be manifestations of a spirit, always or nearly always 'Walter,' mainly exercised by the materialization of organs and limbs for the purpose.

The literature of the case has been extensive, as the following incomplete table, confined to this country, shows.

(1) Hundreds of newspaper articles, including many specially prepared by the inner circle, assailing investigators who have reported adversely.

(2) Many favoring articles in Spiritualist and even some other religious organs.

[*Dr. Prince is the Research Officer of the Boston Society for Psychic Research and holds no academic position; cf. *M.H.V.*, 5 f.—Ed.]

[1] *M.M.*, 13.

[2] *M.M.* gives more or less details of upwards of 260 sittings (see table, 486-499) up to Dec. 27, 1924.

[3] For the period before Dingwall's coming, *M.M.* gives the fullest description of phenomena from its author's point of view; for the meagre but best, printed account of the sittings in the period presided over by Mr. Dingwall, see his address printed in *J.A.S.P.R.*, 1925, 125-134; and for the period of the Harvard Group consult the report of its spokesman, Mr. Hudson Hoagland, *Atlantic Monthly*, 1925, 666-681; also the sitting-notes printed (with important omissions) in *M.H.V.*

[4] Chapters XVIII and XX of *M.M.* give the details regarding the pigeon and the flowers. The phenomena of 'apports' seem to have disappeared from the case before any decisive tests were applied.

(3) Articles of essential advocacy by Mr. J. M. Bird in the *Scientific American*,[5] the verdicts of its Committee members,[6] an article by Dr. Crandon attacking the "preliminary" verdict,[7] and an answer claiming to point out his divergences from fact.[8]

(4) A pamphlet by Mr. Harry Houdini, claiming to demonstrate fraud by 'Margery.'[9]

(5) A book, *Margery the Medium* of 518 pages by Mr. Bird, in defense of her phenomena.

(6) A report of the Harvard investigation, by Mr. Hudson Hoagland, in the *Atlantic Monthly*.

(7) Seventeen statements, discussions and letters relating to the case in the *Journal of the American Society for Psychical Research*, March-December, 1925. Of these 6 were by Bird (made a research officer of the A.S.P.R. following the announcement of his conversion), 3 by Dr. Crandon, 2 by his personal friend; three times the material in favor that there was against, and everything against carefully answered by Mr. Bird or Dr. Crandon.[10] The record shows that fuller details of the Harvard findings were refused although the *Atlantic Monthly* report was assailed by Bird, and that proof offered from outside the group that he had distorted the facts failed to interest the *Journal of the American Society for Psychical Research*.[11]

(8) Lately the Crandon group has gratuitously distributed an anonymous pamphlet of 109 pages, entitled *Margery-Harvard-Veritas*.[12]

The Research Officer of the A.S.P.R. has advocated Margery before many Spiritualist and general audiences, and Dr. Crandon and others have similarly proclaimed her genuineness. I know of no platform propaganda in opposition, aside from the diatribes of Houdini.

It is time that an outline history and critique of the case should be written. This I am attempting on the sole basis of a great mass of printed and manuscript documents. I abstain from mentioning any of the unpublished observations of the phenomena by myself, not acting as a witness or an attorney, but strictly as a reviewer.

I. *The First Year of the Phenomena.* About two-fifths of the 518 pages comprising *M.M.* are devoted to the 133 sittings from May 1923 to April 1924, the materials having been furnished by Dr. Crandon.[13] But as the author tells us that "no record was made of the arrangement of the sitters or the degree of control" at the first 63 sittings, and that such vital information only "sometimes appears incidentally or otherwise" in connection with the 70 which followed,[14] this part of the compilation can hardly impress the scientific reader, however convincing sittings may have been to persons present. Hope quite abandons such a reader when he finds the records mostly chopped into bits, classified and reassembled for dramatic effect under such titles as "The Gobble-Uns'll Git You Ef You Don't Watch Out," and "Ghostly Fingers in the Dark."[15] The anecdotes are liberally sprinkled with the compiler's subtle and somewhat oracular remarks.

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[5] *S. A.*, July and Aug., 1924. [6] *S.A.*, Nov. 1924, Apr. 1925. [7] *S.A.*, Jan. 1925.
[8] *S. A.*, Feb. 1925.

[9] *Houdini exposes the tricks used by the Boston medium 'Margery,'* 1924, pp. 32.

[10] See file of *J.A.S.P.R.*

[11] Both letters of declination and the manuscript of the proffered reply are on file in my office. Bird's article is reproduced in *M. H. V.*, 83-93. I find it impossible to square many of its statements with the evidence.

[12] *M. H. V.* in these notes.

[13] *M.M.*, VIII

[14] *M.M.*, 486, 489.

[15] Sometimes scraps of a sitting are scattered through seven chapters. See table, *M.M.*, 489-491.

II. *The Scientific American Investigation.* The Committee formed by the *Scientific American* to study cases brought to its attention consisted of Dr. Wm. McDougall, Harvard psychologist, then president of the American Society for Psychical Research; Dr. D. F. Comstock, formerly professor in the Massachusetts Institute of Technology, interested in psychical research; Mr. H. Carrington, author of books dealing with psychical research; Dr. W. F. Prince, then research officer of the A.S.P.R.; and Mr. H. Houdini, magician. Mr. Bird became, not by action of the committee, secretary to the committee and, as he says, "stage manager."[16] In the latter capacity he was chiefly responsible for procedure, the strict function of committee members being to act as judges. That bane of scientific research—premature publicity—was precipitated in spite of protests from the Committee.

Never was an investigation more hampered, both at this and subsequent stages, by prohibitions and arbitrary rules.

(1) An investigator ought to be able to see, at least in some kind of light. But the darkness was relieved by red light, more or less, only when 'Walter' gave the signal,[17] reminding one of the magician who withdraws the curtain after a wonder is prepared behind it. Darkness may be required by a 'psychic law,' but it is also convenient for hoaxing.

(2) It ought to be possible to make reasonable use of those important instruments of investigation, the hands. But hands were held fast except momentarily under strict regulations. The 'ectoplasmic limb' may shove, litt and throw objects, slap sitters and pull their hair, and even overturn the table or rip away the wing of the cabinet, but the slightest touch laid upon it unbidden, it is explained, might fearfully shock the medium. This seems unreasonable and yet investigators obeyed the inscrutable law.

(3) Investigators ought to be permitted cautiously to test how far alleged laws are actually valid. An investigator on one occasion ventured to move his own and the psychic's hands slowly into the area between her and the bell-box, and no objection being made continued to do so.[18] Since the bell rang nevertheless, the law against this founded on the theory of 'psychic rods' seemed disproved, yet after-experimentation of the kind was forbidden. If in his one daylight sitting the two momentary muffled tinkles really came from the box and not from beneath the medium's skirts five inches distant, the important discovery was made that the box-bell could be rung under the most favorable conditions.[19] She was uninjured and fresh enough to have another sitting that very night, and four more within a week,[20] but he never could get another daylight sitting.

(4) Scientific investigators are supposed to plan the nature, particular time and duration of their tests. But here 'Walter' autocratically directed, consenting or refusing, unexpectedly switching phenomena or terminating a series of particular type before the hiatus in the proof was filled.[21]

(5) Besides the above traditional ritual restrictive of scientific liberty, the following rules were invented, constituting, whether or not so intended, an effective device to hamper investigators consenting to be so bound, and to embarrass them after an unfavorable verdict.

(a) Every member must sign a blanket report composed of individual observations dictated during the sitting by different members, without a proviso that he is responsible only for his own observations and descriptive terms. The member who declined to assume responsibility for any errors

[16] Mr. Bird in *Banner of Life*, Aug. 8, 1925. See also *M.M.*, 151.

[17] "For fear of injury to the medium no lights shall be turned on at any time without permission of 'Walter'." Draft of rules written by Dr. Crandon, dated Aug. 4, 1924. But Dr. Crandon could turn on white light in the midst of phenomena, with impunity. See *M.M.*, 139.

[18] *M. H. V.*, 9.

[19] Margery's version in *M.M.*, 471-473; Prince's statement in *S.A.*, Feb. and Apr. 1925; *Banner of Life*, Nov. 28, 1925; Dr. Crandon's version in *M. H. V.*, 15.

[20] *M.M.*, 498.

[21] *M.M.*, 26, 161-162, 165, 224, 357, 400.

other than his own has since been voluminously berated for not subscribing to the individual observations of his colleagues since they were colleagues,[22] yet the members have been as vigorously scolded for not repudiating the observations of Houdini, also a colleague.[23]

(b) Another rule, not set forth to any of the investigating commissions, seems to have been in Dr. Crandon's mind. Every particular word or expression dictated during the sittings, no matter how empirical and tentative such necessarily were, especially during the first stages of an investigation, no matter what accidental infelicities of extemporaneous language might have entered, no matter how later discoveries negatived earlier deceptive impressions, must be held sacred forever. There can be no other point in italicising passages of the sitting notes printed in *M.H.V.*

(c) Everything deemed important must appear in the contemporaneous record,[24] the corollary being that nothing not there appearing can ever afterward be deemed important. A more paralyzing rule could not be devised. Frequently facts which at the time seem unimportant and which are clearly remembered by several persons afterward are found to be very important, fitting into the now apparent mosaic. Dr. Crandon and his spokesmen frequently disregarded the absurd rule which they would impose upon others.[25]

(d) The dictated words "control perfect" and the like, shall imply that the possibility of fraud is eliminated;[26] yet thousands have erroneously supposed themselves successfully controlling a medium's movements, and the most intelligent may be deceived for a time. A rule to make a blunder sacred forever or to convict an investigator for becoming wiser is an oddity.

(e) Any observed indication of fraud must be announced immediately.[27] This rule would have the effect of coaching a fraudulent medium and preventing the development of fraud to the point of proof. Houdini's compliance with the rule and the consequent wrangle have been denounced,[28] and so has Hoagland's delaying the announcement until the sitting was over.[29]

(f) At one sitting the investigators allowed Dr. Crandon to dictate into the notes an expression which, as it then read, is rather of a truism: "Unless there is a suggestion of fraud in this record, IT IS *assumed* to be non-existent, *in our opinion*."[30] But by altering this to "if no suspicion of fraud is entered in these notes, it SHALL BE *deemed* non-existent,"[31] there is made a law that an assumption based upon the opinion of an hour shall perpetually have the force of a declaration of fact.[32]

(g) The husband of the medium must be one of her controllers. Exceedingly few of hundreds of sittings were exempt from this condition.[33] Such a situation is scientifically suspect and in view of the fact admitted

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[22] *M.M.*, 454; *M.H.V.*, 9; *S.A.*, Jan. 1925, 65; filed letters by Dr. Crandon.

[23] *Psychic Science*, July, 1925, 127; *S. A.*, Jan., 1925; *Banner of Life*, Oct. 19 and 31, 1925.

[24] Put in writing by Dr. Crandon, and on file.

[25] For one example, see *M.H.V.*, 12. Nothing is in the official notes about Houdini's passing his hand along the medium's arm in the box, though the fact is stated correctly; an "intelligible reason" for the act was given by Houdini at the time. Nor does the record say that Houdini measured the distance from psychic to bell-box at the beginning of the sitting, but it does say that he measured at its *close*. See also *M.M.*, 434.

[26] Put in writing by Dr. Crandon, and on file.

[27] *Ibid.*: "Any sitter believing that he observes fraud shall proclaim it at once."

[28] Houdini's pamphlet, 17 f.; Doyle in *M.H.V.*, 9.

[29] *M.M.*, 23.

[30] Sitting-notes, *M.H.V.*, 43, caps and italics mine.

[31] Dr. Crandon's version, *M.H.V.*, 22, caps and italics mine.

[32] Had the expressions extemporaneously employed by persons guessing in the darkness been thrice as incautious as they sometimes were, to hold them sacrosanct against all after discovery, reflection and analysis, is as intelligent as cabalism.

[33] Dr. Crandon was beside Margery in 109 out of the 112 sittings of the *Scientific American* period whose sitting order was recorded; *M.M.*, 491-496.

in detail even by Bird, that Dr. Crandon's testimony as to continuity of control is unreliable,[34] it greatly increased the difficulty of ascertaining what took place in the darkness.

The restrictions set forth in the above Sections 1 to 4 inclusive show why the majority of the first commission were not able absolutely to determine that no supernormal phenomena were displayed. In the nature of things the restrictions prevented the absolute determination of fraud if fraud existed unless the rules were violated or revealing accidents occurred.

It was by springing the many-toothed legalistic trap shown in Section 5, that an unreflecting or emotional share of the public have been persuaded that the first commission (as well as the two subsequent ones) was inconsistent and vacillating. It is mainly a contention as to words, yet, barring a few colloquial lapses in the notes, the inconsistencies seem those which inevitably attend the progress from a state of mind unable to explain to one able to explain.

Readers of *M.M.* should be warned to take its utterances with liberal grains of salt. Certainly many of them grossly misstate and warp facts of vital importance. A list of sixty selected examples with their refutations from inexpugnable data was offered for Dr. Crandon and the author of the book to defend, without avail, except that the former wrote: "I can probably find more inaccuracies in Bird's book than you can."[35] Hence it is not here necessary to support the warning by citations.

Yet some of the revelations in this book seem naïve. We learn from it and from other sources[36] that Dr. Crandon paid Messrs. Carrington and Bird their expenses for investigating his wife, and entertained the former in his house 44 days and nights and the latter 57. "Houdini and Prince preferred to stop at a hotel." This preference was perhaps on the ground that it would be embarrassing to render a verdict against persons on whose bounty they had been living, and more embarrassing in the event of a favorable verdict to incur the imputation, if not the actual risk, that friendship and knightliness might affect their judgment.

The verdicts of the *Scientific American* Committeemen were as follows, in brief:[37]

Dr. Comstock: "Rigid proof has not yet been furnished."

Dr. McDougall: "As long ago as November, 1923, . . . I was inclined to regard all the phenomena I had observed as produced by normal means. . . Since that date . . . the inclination described above has grown steadily stronger in the main, in spite of some minor fluctuations, and has now become well-nigh irresistible."

Dr. Prince: "No sitting at which I was present was to me convincing. . . . In fact, I could write a chapter of indications which, in the absence of contravening proof, seem to tell the story of normal and deceptive production."

Mr. Houdini: "Everything which took place at the seances which I attended was a deliberate and conscious fraud."

Mr. Carrington: "Many of the observed manifestations might well have been produced fraudulently—and possibly were so produced. . . . But I am convinced that genuine phenomena have occurred here."

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[34] *M.M.*, 157-159, 256; Bird even states this was "usual."

[35] It is proper to quote this sentence, since it is taken from a letter related to the one which Dr. Crandon in *M. H.V.*, 19, opens to the public. "Dr. Prince declares in writing that he can make out an equally good circumstantial case against Margery or Houdini," meaning if the matter of the rule in the box were taken by itself, disregarding the testimony of many persons, printed or on file, that they observed *other* indications of mediumistic fraud, and since he refers to another letter (making Prince back down when it was Dr. Crandon who wrote that *he* preferred "to let it drop"). The letters, as well as the "sixty examples," are on file and open to inspection.

[36] *M.M.*, 155; *M.H.V.*, 9.

[37] *S.A.*, Nov., 1924; April 1925.

Mr. Bird, likewise convinced, and since the most voluble defender, was not a member of the committee.[38] He was, however, one of the two editors who had "worked out a plan," namely the formation of the Committee, to furnish "material which would have to be accepted as authoritative." These are his words.[39]

III. *Mr. Dingwall's Investigation.* Dr. Crandon arranged with Mr. Dingwall, Research Officer of the Society for Psychical Research, to come from London as a sort of appellate court. The great feature of this series, Dec. 1924 to Feb. 1925, was the production of 'ectoplasm,' sometimes like an ill-formed, cold hand feeling like "blanc-mange," and lying on the table or in the medium's lap, sometimes a curious substance seen either issued from or poked into her ear. The substance was never seen in the process of actual materialization. Unfortunately, Dingwall's hopes that the missing links of authentication would be supplied were disappointed, for in his report he says:[40] "The control of the medium appears to be rigid, and is faultless if we accept Dr. F. H. [Dr. Crandon] as a *bona fide* investigator, which, under the circumstances, he is himself the first to admit is impossible. The conditions, therefore, of the sittings are such that I cannot at present affirm my belief in the authenticity of the phenomena."

Dr. McDougall attended a number of these sittings, and there were indications which greatly increased his doubts. Certain photographs of the "ectoplasmic hand," especially, seemed to him to resemble lung-tissue, artificially shaped into resemblance to an ill-formed hand.[41] In at least one the annular bands of a trachea and a tubular opening appeared to be recognizable.[42] Refused prints of the photographs by Dr. Crandon, it was not until a year later that he could submit them to experts. Now Dr. W. B. Cannon, professor of physiology, and Dr. H. W. Rand, associate professor of zoology, both in Harvard University, support McDougall, stating that the 'ectoplasm' undoubtedly was composed of the lung tissue of some animal. Other biologists and physicians coincide with this judgment.[43]

IV. *Investigation by a Harvard Group.* Four instructors and a graduate student—having been somewhat impressed by the phenomena as seen in the Crandon home—formed at 'Walter's' suggestion a circle for study. They secured the use of a room of the Harvard psychological department. The University authorities gave Hoagland permission, if the results should constitite a substantial contribution to knowledge, to submit them and their discussion as a Ph.D. thesis.[44]

A number of professors and physicians were invited to assist, their attendance being limited by Dr. Crandon's requirement that but one or two should be admitted at a time. For six sittings, May 19 to June 24, 1925, no trickery was discovered and the majority of the sitters seem to have become impressed by phenomena. Several reports of the sittings, against the protest of the investigators, appeared in print. The last declared: "It is a joy to observe this study being made by honest men with open minds."[45] But on June 29th and 30th a number of things happened.

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[38]Mr. Bird says he had no doubts of genuineness left after his 12th sitting (*M.M.*, 438) That was Apr. 28, 1924 (*M.M.*, 88ff.). Yet in *S.A.* of Aug. 1924, writing after he had been present at 22 more sittings, he had said: "Manifestations have not yet occurred in our presence under full test conditions."

[39]*M.M.*, 149.

[40]*J.A.S.P.R.*, June, 1925.

[41]*J.A.S.P.R.*, April, 1925, 198.

[42]This photograph has never been published, although evidentially valueless ones have appeared (as those of "spirit" hands, opposite p. 480 of *M.M.*, which might or might not be from the plaster casts shown opposite p. 340).

[43]*N. Y. Times*, Feb. 28, 1926; Mar. 14, 1926.

[44]Statements by members of the group on file.

[45]*Banner of Life*, June 13, 1925, 20, 27; July 4.

Mr. Hoagland, as spokesman of the Harvard group, published a report in *The Atlantic Monthly* of November. Dr. Crandon protested that this was in violation of agreement. On eight grounds appearing in the documents I think this contention unjustified,[46] but, after all, the nature of the facts is not affected by the formal question whether they should have been suppressed. Following is a digest of the principal features in the report.

The great phenomenon was an "ectoplasmic limb" and its acts; but the ectoplasm differed in shape, consistency, reach and many other particulars from that of the Dingwall period.

The report alleges more than twenty items of evidence (some complex), direct and collateral, leading to a verdict of normal production.

Witnesses on the 29th saw a luminous anklet upon the floor; the claim that it fell off because too large was disproved;[47] 'Walter' denied but at the next sitting admitted that it had been off;[48] and it was surreptitiously replaced. Hoagland distinctly saw the silhouette of a foot, its toes holding the luminous disc, and he traced the form of the leg to the knee.[49] The two discoveries gave the clues by which under the same conditions Code was able to reproduce the performances to the entire satisfaction of previous witnesses.

The plasticine, after the 'ectoplasm' had pressed it, showed skin marks, lint microscopically identical with that in the medium's slipper, and also inclusions of sand and an insect skeleton such as would be found upon the floor.[50]

The ankle-bands being firmly secured on the 30th, the leg was out of question, but the controllers (Professor Shapely and Mr. Code) observed the medium work both hands free from control, and one of them detected her conveying objects from the region of her lap and afterward returning them.[51] Internal search of the medium has never been permitted.

The 'ectoplasm' of the 30th differed from that of the 29th by numerous characteristics. Its movements correlated with those of the luminous armlet. Various signs indicated a mechanical contrivance worked by hand. The plasticine received an imprint like that of a chain, but no skin-marks, though there were three deep impressions resembling those which cloth-covered fingers would make. What the feet could not do this evening the hands could do, as Code's duplication under like conditions proved. Thus both on the 29th and 30th the correct scientific method was employed; observation followed by hypothesis, hypothesis followed by verifying check.

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[46]There is space but to allude to the principal of these, viz: that Dr. Crandon formally assented in May to Mr. Hoagland's plan to use the results, if suitable, for a Ph.D. thesis; that no intention finally to suppress them was ever broached; that Dr. Crandon allowed, against protest, data about the sittings to be printed in four issues of the *Banner of Life*; that solely for defense of himself and colleagues against premature gossip Professor Shapley on June 29th stipulated that names should not be given out without specific authorization and that attempts should be made to avoid publicity; that this agreement was not signed by Dr. Crandon and was binding only on those who did sign it; that nine members of the group did authorize giving out a report after the sittings ended; that Dr. Crandon wrote a letter on July 6th saying that he agreed [to Code] not to publish the Harvard sittings except with the consent of the majority of the signers, yet without obtaining such consent he furnished the notes from which a garbled account appeared in several issues of the *Boston Herald*, before the report by Hoagland came out in *Atlantic Monthly*.

[47]The anklets used this evening, for the fourth time, were pinned by the medium to the circumference of her own, were tight enough to leave skin marks, and had the safety pins still in place when they were taken off.

[48]*M. H. V.*, 73, 78.

[49]Hoagland's addendum, expunged from p. 77 of *M. H. V.*

[50]The writer examined the plasticine imprints and found the lint, etc., stamped into the clay, which was clean outside the periphery of the impressions.

[51]Particulars in this paragraph and the next to be found in sitting notes omitted by *M. H. V.* See dots occuring before the signature, *M. H. V.*, 82.

The above, with many other focussing collateral details, left no perplexities in the minds of the Harvard group, including the woman physician whom the Crandon pamphlet attempts to make appear a witness in Margery's favor.[52]

Hoagland's report stresses, at least provisionally, the theory that the trickery is largely automatic, subconscious. But the incorporated argument by Code to that effect can be applied in most of its parts to every professional medium, including those known to purchase their apparatus and to consult with each other as to methodology.

Certain acts and expressions of Code which have been criticised appear to have been effects of friendship for the Crandon family and of his desire to shield it so far as possible, but they only emphasize his reluctant testimony as to the vital facts, which, by the way, do not rest upon his witness alone.

Subsequently, to correct false rumors in the newspapers that they were not agreed, all the members of the Harvard group who could be reached at the time signed and printed a statement that "the group is in absolute agreement that the only conclusion possible to them is that trickery accounted for all the phenomena; that the only possible difference of opinion in the group is to what extent the trickery was unconscious." The signatories were: Harlow Shapley (director of Harvard Astronomical Observatory), S. B. Wolbach, (professor of pathology in Harvard Medical School), Edwin G. Boring (director of Harvard Psychological Laboratory), Hilbert F. Day (surgeon), Deborah Fawcett (physician), Hudson Hoagland (engineer and now Harvard assistant in psychology), S. Foster Damon, Robert Hillyer and John Marshall (all three Harvard instructors in English).[53]

The anonymous pamphlet *Margery-Harvard-Veritas*, lately issued by Dr. Crandon and his friends, attempts to discredit the judgment of all sixteen persons composing three accepted tribunals except Mr. Carrington, and also claims to present the entire notes of the Harvard sittings, "absolutely as they were written and signed."

As regards the first factor, the contradictions, perversions and suppressions of established data are so numerous and similar to those in *M.M.* as to suggest a common hand. It is asserted that after a rule was found in the box Houdini went into "a state of prostration" and exclaimed, "I am willing to forget this if you are," although everyone present knows that the prostration is a myth, and that the words were uttered after 'Walter' apologized for a profane and vulgar sentence which had issued from the region of the medium's mouth.[54] "Dr. Prince . . . declares he does not know whether a bell that lies in his own lap rings or not,"[55] is the twist put upon the incident given above (p. 433) and correctly reported to Dr. Crandon in writing. *M.H.V.* asserts that "there was no luminous doughnut" on June 30th, although pages 80-81 show its use; that "they brought no apparatus except the bell-box" on that date,[56] though various pieces of apparatus are named in the notes printed on the same pages; that the plasticine balls were on June 29th "all piled in a bag" unidentified and "made off with" by Dr. Wolbach,[57] whereas they were carefully marked and placed in boxes. It is asked if the use of the 12-inch anklets on the seventh sitting was "to form a way of escape for the honorable investigators,"[58] although the same anklets had been used at previous sittings.

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[52] *M.H.V.*, 27; see note 61.

[53] "Excessively young men" (*M.H.V.* 83). Their ages range upwards to 40, and the youngest is older than Pitt when he became Chancellor of the Exchequer, Hamilton when made the valued aide of Washington, Huxley when already engaged in the researches which made him a Fellow of the Royal Society, and Crookes when he had begun his important discoveries.

[54] *M.H.V.*, 12. [55] *M.H.V.*, 15. [56] *M.H.V.*, 25. [57] *M.H.V.*, 24. [58] *M.H.V.*, 32.

Secondly, the exact assurance of *M.H.V.* is: "We append herewith *the official notes* of the Harvard sittings. They appear *absolutely as written and signed*" (italics mine).[59] This statement proves, on comparison, to be triply untrue:

(1) *Changes.* The prefatory paragraph on page 40 of *M.H.V.* has been altered, and "Dr. Crandon" or "Crandon" changed more than a hundred times to "F. H." —excusable alterations but for the denial of any changes. There are other alterations, unimportant, but yet alterations.

(2) *Additions.* A paragraph of 108 words on page 82, declaring that in "our minds" is "no doubt that this mediumship is one of the most important ever recorded," masquerades as part of the sitting-notes.

(3) *Subtractions.* Two recorded vulgarisims of 'Walter' are missing from page 40, and a passage of 49 words, containing an especially offensive utterance, has been expunged from page 42. All "damns," references to the Deity and Hades are carefully omitted from pages 43, 45, 47, 61, 62, 66, 70, 72 and 78.

(4) *Omissions.* Now we come to prime offenses, the omission of notes of crucial importance. Although various addenda of the earlier and uncertain stage of proceedings are properly printed, including one written twelve days after the sitting,[60] Mr. Hoagland's addendum of 210 words relating to his discovery of the leg, made during the sitting and announced later the same evening, is entirely expunged, although regularly incorporated with the original official notes and typed on the same page with the matter at the top of page 77! Also, the final 4 1/2 pages of June 30th, partly composed of dictaphone notes of the same evening and partly of addenda supplied by members of the group, embodying their conclusions and in particular vitally important facts of that evening, such as the description by two observers of how the psychic freed all of one hand and the fingers of the other, the use she made of them, the peculiarities of the plasticine prints and other particulars, are completely obliterated, making it appear that all such statements were after-thoughts! Altogether I find that about 1440 words have been stricken out of the official notes of the sittings, although parts of this material, secured from Dr. Crandon, had been printed in the Boston Herald before the *Atlantic* report appeared. On page 82 of the *M.H.V.* version itself the tell-tale dots appear, signifying omission of notes "as written and signed." And on page 27 recourse is had to some of the material, "Notes, June 30, p. 9," which is expunged from the pamphlet, giving the very page of the typed manuscript on which it was found.[61]

(5) *Misdirections.* Italicizing passages nullified by discoveries made after the passages were dictated in good but mistaken faith is as bad as altering the text, since it equally misdirects readers. The most of these passages consist of expressions like "Control O. K." Yet the sponsors for the pamphlet well know that after it was learned that a luminous band could be removed and replaced without detection, and while off still appear in the darkness to be on unless the medium forgot the precaution of partly obscuring it, every previous assurance as to the foot had lost its force. The most flagrant instances of misdirection are where passages similar to "feet and hands visible" are italicized, for those responsible for them know that the luminous bands shed no light on nearby objects and that actually the hands and feet were not visible. The expression is a colloquial one, metonymy for the bands then supposed to mark the exact position of hands and feet. Psychic research is not a game of words but an inquiry as to the facts.

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[59] *M.H.V.*, 37. [60] *M.H.V.*, 59.

[61] Nor does the paragraph correctly represent Dr. Fawcett's testimony. She did not say that "hiding was catamenially impossible" nor anything which necessarily implies that.

Italicized dictations, such as "seems to have very crude fingers," "in general the visible operation of the hand seemed clumsy and awkward," "ill-formed hand," are exactly what the discoverer of the foot in action would desire the reader to attend to.[62]

Those who have the information which explains certain other passages italicize them, apparently, to confuse readers who lack that information. "Foot control checked at Code's request by having each foot move separately in a different direction."[63] As though the free anklet could not be shoved along the floor by the toes! The reader may well be puzzled by the note on page 76 saying that the investigators were to blame for the bands coming off. They at the moment trusted the psychic's statement that the bands were loose—not until afterward did they find that she had pinned the anklets to exactly the dimensions of her own impeccable ones. They should have measured at once, but I have yet to meet the investigator who never forgot anything he should have done. But there are few passages which should puzzle the reader, unless he intuitively has more knowledge than the actual observers who were convinced of particular frauds. In the light of those discoveries many otherwise perplexing notes become not only clear but buttress the discoveries themselves.

The pamphlet puts various questions of such quality as, "If the garter fell off why didn't Code pick it up?"[64] It seems to be held both that the garter did not come off and that it came off because too loose. But it appears that Code was busily engaged in running the dictaphone at the time and, moreover, the rules definitely forbade his doing what is suggested. The garter was off, and this fact was afterward admitted by Walter; did it 'fall' on again? Why, it is asked,[65] did not Hoagland proclaim his discovery of the leg at the moment? And precipitate a wrangle as Houdini did when he proclaimed fraud, and afterwards have it said that he ought to have been thrown out! Of all cases this seems to be the one where an investigator, whether he does a thing or its opposite, is most certain to be asked why he did not do the other thing.

Dr. Crandon continues with extraordinary perseverance to put forward his medium and to attack the lengthening line of accepted and unconvinced investigators.[66] Beyond all other American cases, the scientific investigation of this had been hampered by traditional ritual, "psychic laws" to be taken for granted and attempts to impose arbitrary and irrational rules. So far as investigators consented to such rules they are denounced for the acts or omissions to act resulting from the rules, and so far as any of them declined to be bound by novel and artificial conventions they are denounced for their contumacy. On the other hand there has probably never been a similar American case which has been tried so open-mindedly and even hopefully, so patiently and fairly. Nor, my acquaintance with the documents compels me to add, have any adverse decisions probably been rendered with such extreme forbearance and economy in the use of pertinent material.

The uncommon hold which the Margery case retains upon public attention is, I think, due to several reasons:

(1) The ordinary difficulties of guessing in the dark under traditional conditions aided by a set of regulations, arbitrary beyond precedent.

(2) Uncommon cleverness on the part of the medium, both innate and consciously or subconsciously acquired.

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[62] *M. H. V.*, 54, 56, 62, 63, 67, 69, 71, 73.

[63] *M. H. V.*, 71. [64] *M. H. V.*, 23. [65] *M. H. V.*, 23.

[66] *M. H. V.* indeed sets against the verdicts of the three official commissions the confidence of "three hundred" persons of unknown qualifications who are said to entertain no suspicions (*M. H. V.*, 4.). But so could Cagliostro, Slade and the Davenport Brothers, and so can living and demonstrated fraudulent mediums, point to their hundreds or thousands of believers.

(3) Superior technique in accordance with the psychology of deception; exchanging one type of phenomena for another before the study of the first is complete, and devising new types after a verdict has been rendered, so that the medium always keeps one step ahead.[67]

(4) The exclusion of investigators soon after they begin to express doubts and reasons therefor, thereby limiting the number able to declare actual discovery of fraud.

(5) The personal charm of the medium, winning emotional advocacy, especially among men.

(6) A persistent propaganda, persuading many by its very vehemence that its allegations must be true.

(7) Paucity of response from the opposition, to which the indignities of this kind of controversy are distasteful.

(8) The indifference of the masses to dry logic and analysis, and their instinctive gravitation toward the highly sensational.

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[67] To no scientific commission which has yet reported have the "voice machine" and glass cabinet been submitted. I am therefore debarred by the plan of this article from discussing their modes of operation.

BOOK REVIEWS

The Psychology of Human Society. By CHARLES A. ELLWOOD. New York, D. Appleton and Co., 1925, pp. xv, 479.

Written for use as a text, this book represents the maturation of the author's social theory. Its thesis is that the most adequate explanation of social phenomena is to be derived from the "study of the psychic processes involved in the origin, development, structure and functioning of social groups" (p.16.) This "psychological view of the social life, as essentially a collective behavior process mediated by conscious interstimulation and response, furnishes therefore a basis for the synthesis of other theories, and so becomes itself the sociological view" (p. 466). In regard to method, the author says "A complex science such as sociology demands, for a complete and adequate scientific method, a synthesis of the results of deduction from the principles of antecedent sciences with the facts secured through the inductive study of social life by means of anthropology, history, observation and statistics" (p. 35). It is hardly necessary to point out the difficult position of a science which bases its theory so widely. Even a purely descriptive science would have great difficulty in synthesizing such materials, but where in addition the discipline also seeks to set standards and actually control its subject matter, *i.e.* the behavior processes of society, the obstacles are multiplied a thousand-fold. The purpose of sociology, according to Ellwood, is to determine what social progress is, and to devise the most advantageous means of attaining it. Thus it is a combination of explanatory science and ethical technology. Education is accepted as the proper tool for manipulating society's opinions, standards and behavior; and the interesting statement is made that we may readily prevent the over-development of material civilization at the expense of spiritual culture "by devoting more energy and attention to the spiritual side of our civilization" (p. 450).

The psycho-behavioristic approach is, in the main, general and quite sound. The discussions of instinct, imitation and feeling, and their relation to group life are excellent summaries of the recent theories on these subjects. Particular stress is laid on the modifiability of human behavior, and a long discussion is given the part played by intelligence in group life. The neurological speculations, which are dragged in, are not relevant nor always clear as is well illustrated by the following sentence: "If we include in intelligence the physiological processes of the cortex associated with it, it is surely entitled to be regarded as a phase of behavior and as much a force as feeling and desire" (p. 312). When intelligence is called "a problem-solving ability," one is on fairly safe ground, but to add that it was "developed by the organism when the ready-made reactions of instinct and habit no longer suffice to meet the situation" (p. 313), is teleological to say the least. The fact that intelligence is an adaptive agent does not warrant the conclusion that it was created for purposes of adaptation.

From the position of a layman, it is of value to consider the *a priori* bases of this applied sociology. Briefly, they may be stated as follows: Viewed scientifically, there is such a thing as social progress, and it may be defined as the "increasing of men's capacity to survive, the increasing of their efficiency of achievement, and the increasing harmony of their relations with one another" (p. 424). "The purpose and goal of society is the progressive realization of a perfect society consisting of all humanity" (p. 477). Through the rationalization of social institutions, progress can be effected and science is the method of producing this rationalization.

Although our author evidently identifies "rationality" with social adaptation, he also uses it in reference to the critical attitude toward truth

produced by the growth of science. Thus, he admits that the rationality of an institution can be tested only by the experience of generations and that it would be impossible to determine directly whether it is adapted to human needs. Yet he goes on to say that "the influence of the scientific attitude will ultimately rationalize all phases of human social life," and that by the progress of science we mean that rationality (*i.e.* critical reason) shall permeate art, morality, religion, government and the family life. The desirability of this is at least susceptible to doubt; and it is not a tremendous assumption to identify the scientific attitude with the social attitude, and to suppose that the increase of scientific knowledge assures a better society. Reason is a neutral and just as frequently destroys adequate social adjustments as it aids in their creation. In fact, our author tells us that "the most intellectual people of antiquity, the Greeks, seem to have had little practical social genius, as their social life was characterized from early times by social disunity and disharmony, and at length by corruption and degeneracy" (p. 330). At this juncture, not only do we wonder what "practical social genius" is, but are tempted to ask how it has been "scientifically" demonstrated that the indefinite continuation of a standardized group would be more desirable than a supreme, if temporary, outburst of culture such as that of Greece.

As regards social progress, Ellwood says that it is the business of the sociologist and the social psychologist to favor reforms, such as: the removing of social and economic inequalities which hamper the normal development of the individual; the minimum wage for a "human standard of living;" legislation for protecting the worker against accident, disease and too long hours of labor; in short, the abolishment of industrial poverty. No attention however, is given to the problem of over-population.

As an illustration of the influence of knowledge on social progress Ellwood cites the prohibition movement. He says: "through the accumulation and diffusion of knowledge regarding the physiological and social effects of alcohol, and through the growth of humanitarian sentiments, a great movement has arisen in the more progressive societies of Western civilization which seems about to sweep away the use, if not of all, at least of some of the stronger alcoholic beverages. Let us note, however, that this movement has been successful only to the extent that standards and habits have been inculcated in the group through the imparting of scientific knowledge and the cultivation of humanitarian sentiments" (p. 445). A most surprising illustration, and one which, as every man of science knows, hardly squares with the facts.

No one doubts that there are scientific facts concerning society, hence facts for sociology, but would it not be profitable for writers of textbooks to differentiate between "scientifically established principles" and "social opinion," particularly when that social opinion proposes to mold society!

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Conservatism, Radicalism and Scientific Method. By A. B. WOLFE. New York, The Macmillan Company, 1923, pp. xi, 354.

The author, finding students unable to consider problems in an objective scientific way (because of the stubborn intrusion of emotional and sentimental factors), prepared this book on scientific method with a view of correcting these habits of biased outlook. The book is badly proportioned. It is too large, because of much unnecessary elaboration; and, yet, withal points which are essential to the logical structure of the work are too scantily treated. The consequence is the author because of his brevity on the one hand and verbosity on the other frequently contradicts himself. For example: he commits himself to behaviorism of that liberal sort which does not reject consciousness, and then on later pages proposes such things

as the explanation of 'spiritual' unrest in physiological terms—then in the same breath acknowledges the dualism of restlessness due to an overactive thyroid, and that to a 'conviction of sin.'

The definitions of basic concepts occupy only four pages (pp. 8-11). Sentiment and attitude are hastily defined after citing only two scientists—Warren and Shand. Attitude is defined as "how we respond," yet he speaks of it, not as the response itself, but as a mental attitude (p.12). This confusion of attitude, first as act, then as mental set, is characteristic of the book.

Conservatism is defined as a mental attitude causing an individual to accept things as they are. Radicalism is for a new order of things. These are logical rather than real classes. The worst radical will be conservative in some things. There are varieties and sub-varieties. "Interested conservatism is motivated by narrowly selfish, egotistical, individual or class interests. Disinterested conservatism is motivated by fear, habit and similar 'inertial' factors. That is, though they may be selfish, it is an uncalculating selfishness. This is in effect to deny that conservatism is ever motivated by unselfish impulses—preposterous and untenable in the opinion of the reviewer.

In a chapter on disinterested conservatism we are told that "all conservatism is a safety first attitude, that "conservatism and habit are roughly synonymous" (p. 23). But he now seems to neglect the selfish conservatives who are deliberately and consciously defenders of the existing state. Unless he is willing to go the full length of Watsonian behaviorism, he cannot say that conservatism (if it is a mental attitude) is "roughly identical with habit," but his rejection of Watsonian behaviorism for a liberal sort has already been mentioned. Fear in a social situation is "always essentially a defense mechanism or state" (p. 25). "The animal fears for his life, man fears for his habits." The author forgets that in a large part of our life we fear, not that our habits will be broken, but rather that they will not be broken. Surely, the radical may be afraid that the existing state cannot be overthrown.

It is sheer assertion, out of all touch with historical fact, to say that radicalism is always the outgrowth of personal discomfort. What a warped judgment this leads us to make regarding those Divine radicals, the martyrs of history! But, with his usual inconsistency, our author now modifies this extreme view in a footnote apology to Victor Yarros who has taken him to task for exaggeration. Yarros had suggested that opinions about radicals should be based on inductive study. "There can be no question," says Wolfe, "that the suggested inductive study should be made." Then, with that blissful amnesia which easy-chair empiricists enjoy, Wolfe promptly forgets this precept, preferring a picture of radicalism to suit his preconception to the effect that the motives of radicals are essentially personal and selfish.

The final chapters on scientific method are elementary discussions of scientific method in general. These chapters constitute the constructive part of the book. The reviewer, however, misses in them a discussion of an issue which the book raises. Should such effective factors as emotion, sentiment, wish, be allowed to play no part whatever in forming our social and economic beliefs? If not, why not? In short, if social science is at all normative, if it involves the problem as to what *ought to be*, can this 'oughtness' be intelligently determined by pure reason alone, shorn of the last shred of wish and desire and prejudice?

Wells College C. O. Weber

Anfänge der Reifezeit. Ein Knabentagebuch in psychologischer Bearbeitung. By DR. WILLIAM STERN. Leipzig, Quelle and Meyer, 1925, pp. 125.

This interesting volume is the first of a series planned by the author dealing with the psychology of adolescence. It is a psychological treatment of those portions of a boy's diary written during the period of transition from childhood to adolescence, and the early part of the adolescent period, *i.e.* from the middle of the twelfth year to the close of the fifteenth. Through the analytical study and interpretation of the entries in the diary the author organizes his materials according to their psychological significance, traces the development of adolescent characteristics and interests, and indicates the growth from the restraints of the transition period toward the larger individuality and independence of youth. Quotations from the diary given throughout the book, supply concrete examples upon which the general discussions and interpretations are based.

The author first points out the restraints of the transition period, the range of interests manifested, and then traces the gradual development of self-consideration and self-judgment, the consciousness of growing manhood, and independence, and the realization of sex. In the succeeding four chapters the development of more specific characteristics is given attention: (1) Interest in other human beings as, interest in the observation of people in general, relations with parents and relatives, the forming of friendships and the opposite, and the development of interest in the other sex. (2) Intellectual interests, pedagogical interests both as student and tutor; interests in politics and culture, and in nature. (3) Specific intellectual interests in music, the plastic arts, literature, original composition and religion.

The book is very suggestive as indicating a technique for the psychological study of adolescence based upon diary materials where the student is more likely to find spontaneity than in other forms of written expression. As the writer admits, however, this method of study is limited to that type of youth who makes use of the diary to represent himself as a personality. For the great majority of youth with whom the urge for self-expression does not take this form, it is not directly applicable. The method will have much of value probably, in spite of its limitations, to the whole problem of adolescence; and this little volume of Dr. Stern's will undoubtedly be found useful and suggestive by all students of this phase of psychology.

Cornell University E. N. FERRISS

Determinism in Education. By WILLIAM CHANDLER BAGLEY. Warwick and York, Inc., Baltimore, 1925, pp. 194.

In this little volume of eight chapters Bagley has assembled, with slight modifications and additions, seven papers thrown by him into the unending controversy over the relative influence of nature and of nurture upon the 'intelligence' of man. Several of the papers have attracted wide attention and provoked much discussion—notably the one on "Democracy and the I. Q." and the other on "The Army Tests and the Pro-Nordic Propaganda."

The two chapters devoted to the papers named are, by all odds, the most significant of the volume. In the first the author sets up a fairly life-like 'straw man,' which he then proceeds to rend and scatter, yet does not wholly demolish. In the second he attacks with great vigor and cogency the assumptions, the method, and the conclusions of Carl C. Brigham in his work, *A Study of American Intelligence.* In this attack, to a confessedly sympathetic reader, he appears to be entirely successful. The demolition is complete; and in the demolition Bagley has done much to stimulate a

healthy repercussion against lack of science in use of the only 'science' with which the study of education is said to be furnished, the "science of mental measurement."

But though his forensics are often admirable Bagley's own assumptions, methods, and conclusions are often not scientific. He confesses a 'bias,' and shows it on every page. Frequently he assumes the sort of thing, sometimes the very thing, which he condemns his opponents for assuming. He adopts methods that he disapproves for his opponents, and he builds large generalizations upon evidence as scanty and shaky as that upon which, he charges, his adversaries have constructed the fabric of their "pessimistic determinism." His least controversial and most constructive chapter, that on "Moral Ratings in the Several States," is a first class example of the weakness in assumption and method that he deprecates in others. The chapter on "Education a Creative and Equalizing Force" is also a good example of the chief logical fallacy which he attributes to the 'determinists.' Indeed, to quote his own criticism of Brigham, sometimes it seems that if the "assumptions and methods are valid, the actual facts . . . prove just the reverse of the contention he has set forth."

For the 'pessimistic determinism' which he refutes Bagley would substitute an optimistic determinism. In that philosophy 'systematic schooling' becomes preponderant over 'native intelligence,' the school-master dominates the germ plasm. Since man of the now can undoubtedly do a better job for man of tomorrow than can an alogical and biologic nature, there is hope for democracy. Hence educational determinism is not determinism at all but an optimistic yet 'rational equalitarianism."

Despite the fact that the author appears much more effective in destructive than in constructive argument the book leaves a wholesome impression. Like all of Bagley's books it is thoroughly readable, and in style far above the level of most compositions in the field of education. It reveals continually the quick insight and apt striking power, as well as the faith in democracy and the common man, for which the author is so well and so favorably known. It is having already an influence on the evolution (*absit omen*) of the theory and practice of education.

Cornell University T. H. Eaton

Outline of General Psychology. By R. H. Gault and D. T. Howard. New York, Longmans, Green and Co., 1925, pp. iv, 474.

The authors of this book state in their preface that they hope to appeal to two classes of readers, "to undergraduate students on the one hand, and on the other, to the body of intelligent readers outside of academic circles." We should expect to find, then, not necessarily any contribution to the science of psychology in the way of fact or system but instead a presentation of the familiar material from an angle that will interest such readers. The first four chapters are concerned not at all with the facts of psychology but with those about the organism with which the novice must become acquainted before he can compass adequately the inquiry into the nature and functions of the experience of that organism.

The chapter on attention may be taken as a key to the general treatment of the subject matter. Attention is a selective process. It is not, however, a special process but only "an aspect of the mental process." "To be conscious is to discriminate . . . in a word, to select." The mental and motor aspects of attention and the range, duration, etc., include the usual facts. The 'forms of attention' are extended to cover 'sensorial' and 'intellectual' attention though the only distinction seems to depend upon whether the object is sensory or imaginal. Voluntary attention is given over to 'will,' 'the whips of conscience,' 'the spurs of good or evil prospects,' without any attempt to reduce this 'will' factor to simpler terms. The function of attention for the organism receives its full share of discussion.

It is "to further the ends of human life;" "it is anticipatory—a problem seeking activity;" "it is organic response and adjustment;" and on the physiological side it is "an equilibrium-maintaining process." In the same manner in perception, imagination, memory, thought, emotion, instinct and action psychological, physiological facts are considered with emphasis upon the part that they play in the life of the organism. Such a treatment was forecast in the statement of the aim of the book, and though it will meet with objections from the more strenuous proponents of the several 'isms' it has been carried through consistently. It is disappointing, however, that the facts, etc., taken over from the various schools have not been presented in many cases with their original significance, *e.g.* experimental 'introspection' remains a 'looking within;' Titchener's attentive clearness becomes cognitive clearness. Perhaps these distortions are the natural outcome of the functional-philosophical attitude.

The novel departure and a highly commendable one is the chapter on the history of psychology. Other authors might place the emphasis on different periods but the presence of an historical setting would improve most modern books on psychology. Perhaps due to habituation, one is disappointed to find no references at the ends of the chapters before he finally discovers them at the end of the book. On the other hand it is very pleasant to meet again with authors who will take the time and trouble to head their chapters with appropriate quotations.

Hobart College FORREST L. DIMMICK

Education as the Psychologist Sees It. By W. B. PILLSBURY. New York, The Macmillan Co., 1925, pp. ix, 342.

The volume affords a concise survey of the field of educational psychology. Dispensing with the usual introduction of elementary neurology, the author briefly discusses individual differences and their instinctive basis and then undertakes a consideration of habits, attention, learning, emotion, will, fatigue, and formal discipline. A short chapter has been appended, apparently as an afterthought, on "Tests of Accomplishment in School Subjects."

The chapter on "The Ends of Education" (Chapter VI) serves a helpful purpose in that it presents with clarity what the author conceives to be the aims of education. If there is anything concerning which teachers are uncertain, it is the objectives of their own profession. A discussion which serves to dispel such vagueness is worthy of attention.

The author emphasizes the selective function of education. He says in part (p. 83): "To gather together even half of the men of the upper five per cent of the intelligence of the nation in some two hundred or more institutions and know where to look for them when they are needed is in itself a highly important function. That alone would be worth to the nation what is spent upon the universities, were these men not improved at all by the time spent in the institutions."

The illustrations used in the text are familiar ones from Darwin, Swift, Thorndike and others, and are not in every instance attributed to their sources. At the end of each chapter is appended a list of questions, which are more or less thought-provoking and useful. The references have been selected with great care and their number limited with what the beginning student will no doubt acclaim as praiseworthy restraint.

The frequent application of theory to school problems gives the book a somewhat unusual practical value. It should therefore appeal to the teacher and prove useful as an introductory text.

Harvard University C. E. LAUTERBACH

The Theory of Ethics. By ARTHUR KENYON ROGERS. New York, The Macmillan Co., 1922, pp. 197.

Ethical theorizing, according to Professor Rogers, does not supplant experimental groping toward a satisfying life but it should decrease the blindness of the groping. Guidance of conduct is possible because feelings of satisfaction are indications that we are heading the right way. If when we consider an end we have a feeling of satisfaction, then we must judge by the only criterion we have that the end in question is a valuable one; and if our satisfaction continues while we pursue the end, then our judgment has been a valid one. The 'ought' distinguishing moral value arises when in addition to a feeling of satisfaction there is a feeling of repugnance for a rejected alternative. Apparently the primary factor in such repugnance is our perception that one alternative represents a judgment that is inadequate to the realities of the situation. The account of the genesis of the 'ought' is the weakest point in the book, which contains much keen analysis.

University of Chicago C. M. PERRY

Reason and Morals. By ISRAEL LEVINE. Glasgow. Macklehose, Jackson & Co., 1924, pp. xi, 177.

Psychology, through the insight covered by voluntarism, Freudianism, and behaviorism, has rendered necessary some re-casting of ethics. The point of departure chosen for the present venture is the 'reality-principle,' as used by Freud. Levine seeks to show that in general the moral is to be identified with the reasonable. Reason is the instrument through which such conditions of real life as lie at the base of our coöperative enterprise are discerned and utilized. Through the coördination of the pleasure-principle with objective social demands Freud's insight is capitalized; and through the emphasis upon reason as the medium of adaptation contact is kept with the idealistic tradition in ethics. It is not probable that all the relatives of either contracting party will be entirely pleased with the union; but the end proposed is valid and the analysis is well sustained throughout.

University of Chicago T. V. SMITH

The Problem of Immortality. By R. A. TSANOFF. New York, Macmillan Co., 1924, pp. 418.

The book is a scholarly treatment of the problem of immortality from the standpoint of the philosopher. He traces the history of the problem from primative thought in its various philosophical and religious phases through the modern period. The book is essentially a philosophical treatment and may, therefore, be disappointing to one primarily interested in the psychology of the belief in immortality. The last chapter of the work consists of an able discussion of the philosophy of value, personality and destiny in a modern metaphysical setting.

Harvard University HUDSON HOAGLAND

The Unconscious: An Introduction to Freudian Psychology. By ISRAEL LEVINE. New York, The Macmillan Co., 1923, pp. 209.

Within the covers of this little book the author makes the modest attempt to discuss the pre-Freudian treatment of the unconscious by Leibnitz, Schopenhauer, Maine de Brian, Hartmann, Fechner, Nietzsche, and Butler; to outline Freudian doctrine; to present a defense of the unconscious as a psychological hypothesis and of the necessity for writing a psychology in purely psychological (non-physiological) terms; and, in addition to all this, to describe the implications of psychoanalysis for education, crowd psychology, ethics, aesthetics, and philosophy, with general emphasis on the

light cast by psychoanalysis on all fields of human endeavor and interest.

Although the author emphasizes the contrast between Freud's scientific conceptions and the metaphysical character of all previous discussion of the subject of the unconscious, he has thought it unnecessary to adduce any evidence of the scientific nature of Freud's work. He assumes the validity of all the Freudian concepts in the discussion and considers any question settled when he has found an appropriate quotation from Freud.

Harvard University E. C. GILBERT

Psychoanalysis: Its Theories and Practical Application. By A. A. BRILL. Philadelphia, W. B. Saunders Company, 3d ed., 1922, pp. 468.

If one wishes to know what psychoanalysis is about and seeks to learn the Freudian principles and practices, set forth within the compass of a single volume, Brill's book very well suits the demands. This edition, the third, has been thoroughly revised and enlarged. Brill feels that despite the tremendous success of psychoanalysis, and "disregarding the foolish ranting hurled at psychoanalysis now and then by ignorant individuals," there is too much misunderstanding among those who take the subject seriously. To correct this 'grave' situation he has added two new chapters, one on 'Homosexuality' and the other on 'Paraphrenia.' Many new cases and illustrations are added to make clearer the precise applications of the Freudian theories.

Cornell University F. LOVELL BIXBY

How Much English Grammar? By MARTIN J. STORMZAND and M. V. O'SHEA. Baltimore, Warwick and York, Inc., 1924, pp. 224.

If we would keep our textbooks fresh and virile, we must, in the opinion of the reviewer, from time to time take stock of the actual needs in the fields concerned. For in certain instances, perhaps, there is too little relation between textbooks, classroom instruction, and current practice. But, on the other hand, we must not drift so far with the current as to lose the richness of the past.

The authors have undertaken a comparatively exhaustive inquiry with a view to learning what grammatical constructions are commonly used by American students and adults in their written expression. Ten thousand sentences were selected from material of certain grades of current usage. Upon the basis of the frequency of usage and frequency of error, as indicated by this investigation, this little volume points out in detail what should be emphasized and what may safety be eliminated in English grammar today.

The form of presentation is simple, condensed, and intelligible; and the book should be of considerable value and interest to those who are concerned in planning or teaching language and grammar courses in elementary and high schools.

Cornell University H. D. POWERS

The Intelligence of College Students. By ANDREW HAMILTON MACPHAIL. Baltimore, Warwick and York, 1924, pp. 176.

This book presents a survey of present methods of selecting college freshmen and tests their predictive value as found in a large number of colleges and universities and at Brown University in particular. Numerous facts and conclusions valuable to the college administrator are found in the book. A bibliography of 294 references is given at the close for those who wish to pursue the subject further.

Cornell University J. P. GUILFORD

NOTES

Dr. Davies on "Mechanism, Meaning and Teleology in Behavior"

Dr. Davies, in his article on "Mechanism, meaning and teleology in behavior,"[1] discusses the topics with reference to my own point of view as expressed in my paper, "Behaviorism and behavior."[2] Some of his criticisms are anticipated in my book on human behavior[3] which, however, had not appeared in time to be available to Davies.

Before turning to the details of his criticism I wish to meet some of the general questions which Davies has raised against my materialistic determinism, and against the restrictions which I seem to place upon what he regards as the 'realities' in life and mind. He regrets that I do not "admit to the world of existence . . . anything that falls outside . . . the 'movement continuum'." "One would have thought," he continues, "that questions of reality were sometime in their discussion questions of fact, and it is the question of fact that I find assumed in a good deal of behavioristic discussion" (p. 8). I can see no scientific problem in this quotation. The chemist in his laboratory never asks himself or any one else: Is this which I am observing 'real' or not 'real'? Is it a 'fact' or not a 'fact'? Again when I observe and record an infant's movements it never occurs to me to ask: Are the stimuli which are acting 'real' or are they 'facts'? Even when I record my own responses to a stimulus and then (using my own responses as stimuli) record the responses to my responses (even to the third or fourth degree) the question of 'factuality' or 'reality' never arises. Of course both the chemist and myself may doubt whether our records or measurements are reliable or whether our sense organs are functioning normally, but we may determine the degree of reliability by the degree of correlation which exists between various individuals making the same observations. Davies' facts and realities are not the observable objects or relations between objects. They are verbal responses characteristic of individuals who have been raised under what we may call a philosophic environment. From the scientific point of view they are merely stimuli produced by movements of the speech mechanisms which may or may not be investigated with respect to their own sensorimotor antecedents. Philosophers have no sense organs which the physicists do not have. They cannot see a 'reality' which the physicists are unable to see. The 'facts' and the 'realities' of the philosophers are merely names for the way in which objects, movements, stimuli, responses, etc., are classified by the philosophers. The scientists may accept or reject this classification, or substitute one of their own, but whatever the classification the objects, movements, stimuli, responses, etc.,remain unchanged.

That, according to Davies, I should logically accept the syllogism: "Everything that is real is physical; secondary qualities, if they cannot be ignored, are real; therefore, secondary qualities are physical" (p. 9), does not disturb me particularly. The syllogism is merely a series of verbal responses for Dr. Davies and a series of visual stimuli for me. The scientific importance of the syllogism depends upon whether (as a verbal stimulus) it leads scientists to discover new properties or new relationships between properties. Having such a simple test I fail to be impressed when Davies informs me that "a very considerable portion of the time and energy of a good many of the leading minds in this country and in England, during the last twenty years, have been given to the discussion of just such questions. In fact, the modern form of the realistic controversy may be said to center upon the status of the secondary qualities" (p. 9).

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[1] *Op. cit.*, this Journal, 37, 1926, 2-24.

[2] *Op. cit.*, *Psychol. Rev.*, 31, 1924, 32-50; 118-149.

[3] *A theoretical basis of human behavior*, 1925.

It occurs to me that for many years philosophers and "a good many leading minds" spent their energies arguing about the number of angels that could stand on the point of a needle. When physicists or chemists discover primary or secondary qualities, or when they accept the classification as a useful working hypothesis, it will be time enough to introduce the syllogism into the study of human behavior. It remains to be seen whether my statement "human behavior is a form of motion" is a more effective stimulus for analyzing out the elements of human behavior and achievement than the stimulus "you must not fail to look for reality" which, it seems to me, represents in a condensed form Davies' caution against extravagant behavioristic methods.

We pass to a consideration of consciousness which Davies maintains is not adequately considered. He says, "Whether 'consciousness' could or could not sustain a rigid examination as to its factual existence is not now being considered; what I am saying is that the importance and even necessity of making such an examination has been obscured by reason of the position that behaviorists and others have taken with respect to questions that have no necessary relation to this one. Mechanism, as I have pointed out, is one of these; the 'movement continuum' is another" (p. 8).

When I am most active in adjusting myself to my environment there seems to be no need for me to 'remain conscious,' to 'apprehend clearly,' to 'develop a consciousness of the *meaning* of what I am doing,' etc. It is only *after* a series of adjustments have been made that I may enter upon an entirely different type of activity, that of *classifying* the movements or partial movements that make up the adjustments. In this I may (a) use the traditional mental classification, or (b) classify the adjustments as forms of movements, reflexes, habits, etc. The two classifications I regard as mutually exclusive and the first one (a) as scientifically unproductive.

Whether mechanism or the 'movement continuum' has any necessary relation to consciousness will of course depend upon what we are to include under the term. That there are many things that might be (and have been) included, I do not deny, but at present I feel no more secure in trying to decide on the content of the term (consciousness) than does Davies himself, who thinks it should be 'dropped altogether.' I have gone somewhat further and 'dropped' all terms of this type, even the 'mental order' which Davies proposes as a substitute for consciousness. Davies, of course, does not approve of this, for he says, "what we mean by an image is the apprehension of a material object when that object is outside the range of our perceptual, that is, receptor-effector system. Does the behaviorist have to be in physical contact with the object in order to describe it or react to it? Or can he describe and react to it in its physical absence? If he can, how is this possible unless he apprehends it in some non-physical fashion?" (p. 8).

The term 'apprehension' in the domain of science can only be a verbal stimulus which releases a series of more or less circumscribed responses. That it releases a different set for every psychologist who is asked to report what he means by the term can only be a justification for dropping it. Its vagueness does not demonstrate the existence of non-physical relationships. It is only when imagery is forced into such categories as 'apprehension' that we get the apparently absurd condition of reacting to a non-physical condition. If I am asked at this moment to describe Mr. X whom I have not seen for five years I believe I can make my description accurate enough so that Mr. X might be selected from a group of say ten persons. The fact that I have not seen Mr. X for five years does not mean that I am now "apprehending him in some non-physical fashion." It only means that I am now reacting to the verbal stimulus, 'describe Mr. X,' in the same way that at one time I did react to Mr. X in the flesh. That I am unable now to fill in every physical and physiological process which has occurred during the last five years, does not require me to assume that it cannot be done or that any non-physical conditions have been operative. Of course I have

learned to speak about the association of ideas, the mental processes of recognition and recall, as have other psychologists, but how these ideas, recognitions, recalls, can actually bring about the movements in my fingers by which the description of Mr. X is written, I have not learned. Futhermore, when I try to describe along traditional lines, what I have been taught to call an image or a mental process, my own descriptions are just as variable as those of others. The more critically I observe my own so-called mental states, the more they differ from each other and from those of other psychologists.

We may now consider the specific questions which Davies raises.

Mechanism in Behavior. The following quotations deal directly with the problem. "The advantage of approaching the problems of behavior from the standpoint of biology, rather than from the standpoint of physics, is that we are not thereby committed inevitably to the theory of mechanism. It is true that one of the aims of science is prediction and that predicting implies uniformities of some sort, but this may be granted without begging the question as to the kind of uniformities that are involved in human and animal behavior" (p. 10). "One would have to be very forgetful of the present-day situation of biology to say that the acceptance of a biological point of view commits the psychologist to the position of materialistic determinism" (p. 11). This quotation frankly proposes a limited determinism, which, of course, is indeterminism. Recent statements by biologists, such as C. M. Child[4] and R. S. Lillie,[5] place protoplasmic processes definitely in the series of physico-chemical processes. This would make biological mechanism one with physical mechanism. I would not deny that we may always have a limited *predictability* in human behavior, but this does not mean limited *determinism*. We also have limited predictability in physics and chemistry. Thus the meteorologist is unable to predict the weather accurately, but he does not ascribe his errors to a non-material cause. He merely has not yet isolated and measured the essential *mechanical* causes of the weather. This does not, however, lead him to indeterminism, but actually leads to a more rigid mechanical determinism.

Where it is possible to use statistical methods in biology we have the same type of evidence for materialistic determinism. Thus in vital statistics and life insurance it is possible to predict within a rather narrow range what percentage of the population will die, say, between the ages of forty and fifty; the relative proportions of the deaths from heart, respiratory, or intestinal causes; the number of automobile accidents; etc. If we take such social statistics as school enrollment, theatre or church attendance, number and kinds of books bought, library attendance and kinds of books called for, we again find a degree of predictability which points to mechanical determinism, not to non-mechanical causes. Even such measurements as intelligence tests, educational tests, tests of mechanical or musical ability, all point to mechanical causation when the number of measurements are large enough to establish norms. Of course, since human behavior is more complex than that of the weather or the amoeba, we would expect less accuracy in the prediction of the behavior of man.

The 'be careful' attitude which Davies recommends is, of course, the proper attitude for science; but for science, 'be careful' *now* means, "assume natural (mechanical) causes until compelled to do otherwise." For traditional psychology and philosophy, however, 'be careful' still means, "do *not* assume natural (mechanical) causes *until* there has been a *complete* demonstration that they are adequate." The normal response to such hair-splitting is, "Well, what difference does it make?" As I see it, the difference revolves about the practical effectiveness of a working hypothesis of the 'Thou shalt' type as compared with one of the 'Thou shalt *not*'

[4] C. M. Child, *Foundations of Physiology*, 1924.
[5] R. S. Lillie, *Protoplasmic Action and Nervous Action*, 1923.

type. That is to say, actions and investigations which result from the 'Thou shalt' form of stimulus can be measured and bear directly on the problem under investigation; action which follows upon the 'Thou shalt *not*' type of stimulation, while it may be measured is usually irrelevant to the problem being considered. I have adopted the working hypothesis, "human behavior is a form of motion" because it is of the 'Thou shalt' type. I justify this rather premature hypothesis because I think it is time to investigate human behavior and human achievement by methods that will lead to something which can be made the basis of social science. I do not, of course, subscribe to any 'psychology for itself alone' sentiment.

Psychology should forge ahead of even the biologists in placing human behavior upon a scientific (and this means mechanistic) foundation so that social science may derive from the investigations of human behavior the same sort of support that biology now receives from biochemistry, and biochemistry in turn receives from physics. In this way we have the possibility of a mechanical series ranging from the electrons to the League of Nations. If mechanical monism has made physics, chemistry, biology what they are, perhaps it is worth while trying it in psychology and eliminating even the attenuated dualism which still lingers and which Davies urges us to retain.

Meaning in Behavior. The central problem of meaning as Davies sees it, is 'complication.' "Complication in human experience and behavior is," he says, "only another name for development and, if we so take it, it may not be difficult for all of us to agree that a primary condition of any profitable discussion is that the problem be broken up into a number of constituent questions, and that each be given a careful definition" (p. 12).

When he introduces 'development' as an aspect of human behavior, I think he is touching on a fundamental principle of organization which passes beyond any limited psychological range and participates in all scientific investigation. Grant this, then, is it not better to classify this phase of human behavior with the other scientific fields rather than create a new continent for such an ambiguous term as 'meaning'? As Davies points, out 'development' refers to some form of interaction between the organism and its environment, but even the biological conceptions of the terms are restricted to the field of organic relationships and we have failed to consider the possibility of human behavior as belonging to a wider category which includes all changes, inorganic as well as organic. If the terms organism and environment are extended beyond their biological limits we may construct a series of the following order: Let the term organism include the whole solar system (of which our sun is the center) then the term environment would include all the planets, stars, and everything else beyond the spatial limits of the solar system. If we start with the earth as the organism the rest of the solar system and everything outside of it is the environment. Starting with a human individual as the organism—everything outside of the individual is the environment. Starting with the nervous system of the individual as the organism—the other organs, tissues, fluids, the earth, sun, stars, etc., are the environment. With a single nerve cell as the organism—the other nerve cells, organs, the earth, etc., are the environment. With the nucleus of the nerve cell as the organism—the cytoplasm, protoplasts, cell membrane, axone, dendrites, other nerve cells, etc., are the environment. Using a chromosome of the nucleus of the nerve cell as an organism—the other chromosomes, nucleoplasts, etc., are the environment. Using a chromomere as an organism—the other chromomeres, chromosomes, etc., are the environment. With one of the chemical compounds in the chromomere as an organism—the other compounds, chromomeres, etc., are the environment. With one of the molecules of the chemical compound as the organism—the other molecules, etc., are the environment. With one of the atoms as the organism—the other atoms, molecules, etc., are the environment. With one of the electrons of the atom

as the organism, the environment becomes (in a 'house that Jack built order') all the other electrons, atoms, molecules, compounds, chromomeres, chromosomes, nucleus, cytoplasm, cell membrane, nerve cell, nervous system, bodily structures and organs, the human individual, the earth, moon, sun, solar system, stars, and the universe. This is about as far as we can go at present.

The distinction between organism and environment is always an arbitrary distinction. We may include the atmosphere of the earth as part of the earth or as part of its environment, depending on the nature of the discussion. The important thing to realize is that the distinction is arbitrary and in no way affects the balanced mechanical interactions between the parts of the system. One changes the other, and the other changes the one. The question which Davies raises, reduces itself to, "Shall we mark off particular limits in this series of reciprocal interactions and say that within these limits we will refer to the interactions by the term 'meaning'?" As an expedient, science requires a specialization of labor but it does not require different names for the same thing. The term 'meaning' is not used very generally as a scientific term. In fact its implications are obviously non-scientific. While I agree with Davies that 'complication' in human behavior is an important topic for investigation, it seems to me that the physical, chemical, anatomical, and physiological terms and methods which we already have, will lead to a social science more rapidly than any attempt at revising and re-defining the subjective classification which had its origin, not in science, but in poetry, mythology, and an egocentric theory of human behavior.

The more specific problem of meaning Davies states as follows: "the problem of meaning arises when the environment presents a complexity that goes beyond that which is characteristic of the situations in which the fundamental reaction-patterns have evolved. From this point of view, behavior is said to have meaning when the adjustment has some obvious and statable relation to this complexity" (p. 13).

I agree with Davies that behavior does have "some obvious and statable relation" to the complexity which arises when new stimuli act upon old reaction-patterns. But this relationship I include under the category of learning and habit formation instead of adding it to the muddle of definitions which have already been assigned to the term 'meaning.' Such an act as crossing a crowded street, Davies maintains, "refers to a situation which lies outside the neuromuscular changes to which the mechanistic behaviorist would reduce all behavior whatever, and" Davies continues, "the neglect of this feature in his descriptions obscures for the behaviorist an important problem in his explanation of how behavior is possible" (p. 14).

This is not the place to review experiments on learning, the variation of response, or the conditioned-reflex, of which crossing a street is an example. To convince oneself of this it is only necessary to watch a benevolent policeman helping an elderly country woman, who but for the assistance in crossing, would stand rooted to the sidewalk 'neurally blocked.' The illustration is far too complicated for a behavior analysis and since Davies himself makes no attempt at analysis in terms of his category of 'meaning' I wish only to assert that in learning to cross a street, I see nothing essentially different from learning to dance. Instead of 'obscuring an important problem' (new responses to new situations) as Davies maintains, the behavioristic method is responsible for what light exists.

"I have questioned," says Davies, "if an adequate analysis of perceptual behavior has been made by behaviorism, and the conclusion seems to be warranted that an element has been omitted that, for the purpose of reference, I have called 'meaning' " (p. 17). To this I would answer that for the philosopher the term 'perceptual' is 'loaded' in such a way that it already includes a non-mechanistic factor, and I do not believe that any analysis would be acceptable to him unless it did include a non-mechanistic

element. I merely maintain that when I see (perceive) an object and respond to it, I find nothing which seems to require a category of 'meaning.' For instance, I never find myself observing, that the pattern of white and black circles before me, *mean* a cup of coffee, or *mean* a sleepless night, or *mean* the proximity of a speech. I merely *respond* to the stimulus by drinking, or not drinking, placing my napkin on the table, pushing back my chair, or in the many other ways in which I have responded to a cup of coffee. Of course I may afterwards review the action and analyze out the stimuli, anatomical structures, physiological processes, the change in the environment which my movements have produced, and then classify these steps. That it is as yet impossible to make a classification in terms of a complete regression of the physical antecedents of every stage in the responses, to whatever my infantile sensorimotor equipment may have been, I will also admit, but this implies only a lack of technical ability to do so. It does not imply that there is a *meaning* element which somehow or other has eluded and will always elude a scientific analysis.

Before I am willing to accept Davies' conclusion that "some kind of a criterion is necessary to determine the subject matter of behavioristic psychology" I should like to know more about the specific criterion that is established. So far as 'meaning' is a topic in current systematic psychology it seems to me to have reached the same level as 'consciousness.' To drop it entirely, seems the best alternative.

Teleology in Behavior. That I have not made clear what I mean by 'social' behavior appears from the following:

"Weiss skillfully avoids the question between the leading schools of social psychologists, and between them and the sociologists, as to what constitutes 'social' behavior, but he does so at the cost of giving us merely a negative statement of what he means by 'biosocial equivalence' " (p. 17).

In the more complete account in *Theoretical Basis of Human Behavior*, I have defined human behavior as those movements of the individual which produce changes in the environment (stimuli) which release responses (in others) that indicate the individual's social status. This is, of course, an arbitrary differentiation in the interest of the study of human behavior and is designed to exclude the more purely physiological processes of digestion, respiration, regulation, circulation, sleep, etc. Positively I characterize these movement complexes as the educational, vocational, administrative, recreational, and personal responses of the individual. I name them *social* behavior because the conditions under which they are learned include stimuli which operate *only* when there is a sensorimotor exchange between two or more individuals. That this behavior goes beyond the limits of heredity in the sense that 'the lone child on a desert island' would never acquire the conventional responses or learn to produce the stimuli, is obvious. The origin of 'social' stimuli is through a historic and progressive series of sensorimotor interactions between many individuals. Thus, oral speech, whatever may have been the original conditions under which it actually did develop, now represents a condition in which some sound made by individual A becomes a *substitute* stimulus for some object which acted on the eyes of A but not on the eyes of B. This substitute stimulus (usually verbal) releases in B the appropriate response to the stimulus which only acted on A. By sensorimotor interaction between individuals I mean merely that B's responses are appropriate responses *to what A saw*; B has used A's eyes instead of his own. Thus when A reports (in substance) to B, "I just saw a man-eating tiger headed in the direction of your hut," B does not respond to the cadences, rhythms, vocality, and tonality of A's voice, but he responds as if his own eyes had been stimulated by the man-eating tiger.

A discrimination between (a) biophysical stimuli, (b) biophysical reactions, (c) biosocial stimuli, (d) biosocial responses, should help in comparing them with each other.

(a) A biophysical stimulus is any form of energy which releases function in a sense organ; light, sound, pressure, warm, cold, etc.

(b) A biophysical reaction begins when sensory tissue is stimulated and a nervous process is transmitted over afferent, central, efferent neurons, to some motor organ or gland cell. The biophysical reaction ends with the contraction of a muscle or the secretion of a gland. All biosocial stimuli and responses are biophysical stimuli and reactions, but in addition they are *substitutes* for stimuli and reactions which have occurred in the past.

(c) The biosocial stimulus is one that has a social history. It is a residual effect of the interaction of two or more individuals, and has its origin in social interaction. Thus the statement, 'human behavior is a form of motion' is a biosocial *stimulus*. As it appears on this page it is a pattern of reflected light waves which act upon the eyes of whoever reads this paragraph. It is a substitute stimulus for the original stimuli which acted on many individuals while they were responding to (observing) the actions of other organisms, and a substitute for the recording responses made to the original stimuli. In what way the stimulus will affect the reader, will depend upon the social history of the reader. If he has not learned to read English, it will have practically no effect. If he has learned to read English the effect will depend upon what books he has read, his education, his verbal habits, the modifications which have occurred in his sensorimotor system. How much all of this depends on social interaction we need not attempt to analyze. I merely wish to add that a given biosocial stimulus may assume a number of different physical forms as, oral, written, printed, engraved, code, shorthand, etc.

(d) The biosocial response is a series of motor or contractile effects which produce the biosocial stimulus. The statement 'human behavior is a form of motion' is also a biosocial *response*. It has sensory, cerebral, and motor antecedents just as the biophysical reaction, but the motor component has produced a change in the environment (the light pattern of the printed statement) and this change is a 'socialized' stimulus for others. Obviously the statement could not have been produced without the interaction and coöperation of many individuals, living and dead. It has a social history in that it resembles other responses produced by other individuals. It also has an ontogentic history in that there was a *first* appearance of the response in the life of the one who first produced it, and its first appearance had sensorimotor antecedents. That it is difficult to establish these resemblances and antecedents goes without saying, but this is what the study of human behavior and of human achievement is for.

The dual character of biosocial stimuli and responses is better brought out in the following illustration. "Will you take dinner with us?" for me, is a biosocial *stimulus*, but for my friend who extended the invitation it is a biosocial *response*. As a biosocial stimulus or a response its particular biophysical form, whether oral, in writing, by telegraph, or by telephone; or whether I respond to the stimulus orally, in writing, by telegraph, or telephone, is unimportant. The physical forms of stimuli or of responses are *biosocially equivalent*, because the behavior of my host and myself at the dinner will be the same whether he invited me over the telephone and I accepted by mail; whether he invited me by telegraph and I accepted by long distance telephone; or whatever the combination may have been. It is in the sense that stimuli and responses which are biophysically different may be *biosocially equivalent*.

Perhaps I have stressed the difference between biophysical and biosocial *ad nauseam*, but I wish to clear myself of the charge of 'camouflage' which Davies makes. If the statement, "Will you take dinner with us?" releases the *same* response, whether the statement reaches the ear as a complicated sound wave produced by the muscles of the vocal mechanism, or reaches the eye as a light pattern produced by the entirely different muscles of the writing hand, then I have made my point of the biosocial

equivalence of two stimuli which are biophysically non-equivalent (incommensurate). It remains only for me to show that this can be done *mechanically* (biologically) to meet Davies' criticism. This I have tried to do by indicating that the biosocial stimulus is a *substitute* stimulus for some other form of stimulation. Thus the social antecendents of "Will you take dinner with us?" were probably first developed as pantomime. The verbal forms are merely substitutes for this pantomime. I indicate specifically in my book,[6] the mechanical principles by which the word 'orange' may become, in a social sense, the substitute for an actual orange, and generally how the process of 'substitution' may be resolved into mechanical antecedents.

From the following quotation it seems to me that Davies arbitrarily places all social interaction into a teleological category. "But not to press the point, one does begin to wonder, in the light of the recognized discontinuity between biophysical and biosocial behavior that is positively affirmed in Weiss' denial that these two classes of phenomena are different aspects of sensorimotor organization, what mechanical explanation of the biosocial can be given that escapes the charge of being essentially teleological" (p. 18).

If Davies insists that all social or biological reactions are teleological (by definition) then, of course, mechanism is automatically excluded. The final criterion of teleology which Davies proposes is not stated clearly enough. He says, "Ultimately, teleology rests upon an affirmative answer to the question: does the behaving individual contribute factors to the conditions under which behavior takes place so that, if all other factors remain the same, the behavior would, in this case, be different?" (p. 20).

My answer to the question is, yes, the individual does contribute something, but this something is mechanistic. In the classical 'burnt child' illustration, the second time the child responds to the candle it responds differently than it does the first time, because after the first response the sensorimotor mechanism of the child has become so modified by the first response that the child is no longer the same. The child's contribution is this modified sensorimotor system. However we find the same thing in geology. Does not a stream modify its own channel through the mechanical action of the water on its own shores and stream bed and *contribute* this modified channel to the effects which next year's flood will produce? Knowing how the child responded to the first sight of the candle, is it not easier to predict how it will respond the second time, than it is to predict how this spring's freshet will modify next year's stream bed? To differentiate the action of the 'child' from the action of the 'stream' and to say that one is teleological and the other mechanical, seems to me to be merely another attempt to preserve philosophical distinctions where science finds none.

I have already indicated that the statistical treatment of human behavior points toward mechanical causation in social laws. Davies' 'indeterminism' means only 'low predictive value,' and this prevails in what is regarded as strictly physical (the weather, earthquakes, tidal waves, etc.) quite as much as in social or biological occurrences. Even the most complicated forms of behavior may be regarded as one stage in a strictly mechanical series. For instance, from my postulates[7] on 'Stability and Form' and 'Organization' it follows that human behavior is one of many forms of motion occurring in an electron-proton aggregate. Variability in duration is one phase of this variety. In its simplest forms human behavior is made up of movements which have survived in the reciprocal interaction between the organism and the environment. In man these movement complexes and interactions have reached a maximum variability. That some of these complexes should assume the form of what I call sensorimotor

[6] *A theoretical basis of human behavior*, Chapter XIII.

[7] *Op. cit.*, 390-391.

interchangeability between individuals, is only one of the many probabilities which might be expected from the law of like and unlike signs. That the still more complex forms of this sensorimotor interchangeability that I have called *compound* multicellular organization increase the stability of those movements which remove the limitations of disease and death; extend the sensory range and enlarge the environment; extend muscular energy and precision through machinery; correct faulty inheritance through education; reduce the time and energy devoted to the food and shelter activities; eliminate wasteful competition between individuals; is again only the mechanical operation, within specific electron-proton aggregates, of the law of like and unlike signs.

We may express all this in such a statement as 'the behavior of man is being modified in the direction of producing an optimum relationship between (a) the time devoted to the production of wealth (in the economic sense) and (b) the time available for recreative or leisure activities (productive or unproductive). The most effective method in the production of wealth is through coöperation and the specialization of labor, but such movements require the individual to adapt himself to a tempo, a distribution of his time, and a rate of output, which is set by the *group* or by machinery. This tempo and rate is always too fast or too slow for any given individual. Recreation on the other hand is made up of movements whose tempo and rate is regulated by the sensorimotor condition of the individual rather than by social practice or machinery. There is thus an antagonistic relation between 'labor and leisure' which is constantly approaching an 'optimum ratio' which varies from generation to generation, from nation to nation, from infancy to old age. That we should expect just this type of behavior from the operation of the law of like and unlike signs would justify the philosopher in classing behavior as teleological because it is in conformity with this 'optimum ratio' or *final* tendency, but for the same reason the scientist is justified in calling it mechanical. This paradox leaves the questions (a) shall we extend the philosophical conception of teleology *downward* into the sciences from which every attempt is being made to eject it, or (b) shall we extend the scientific conception of mechanism *upward* into social interaction. When we have actually studied how biosocial stimuli and responses develop, I do not think we shall need the teleological category. That I propose to throw out this category first rather than last is a personal response arising from the observation that practically no advance was made in physics, chemistry, and biology, until teleology was thrown out.

In defining human behavior as a form of motion I, of course, leave no room for what Davies calls uniformities of some sort or other which do not commit the psychologist to the position of materialistic determinism. But why should this spectre of vitalism be a constant source of inhibition when not even a half-dozen psychologists will agree on what is to be included under any one of these non-physical uniformities? That we can continue to describe human behavior in the old subjective terminology I do not deny; and that it is possible to improve this so that it will more adequately describe human behavior, is also granted—but it is not being done. Every critic of mechanism in human behavior instead of improving the subjective terminology which already exists merely adds a few more categories. Davies' wishes, McDougall's purposive strivings, the drives, meanings, desires, various dynamic and teleological factors which have sprung into existence during the last twenty years should be a warning to one scientifically trained. My assumption is only a working hypothesis to be used as long as it works. An attitude of doubt, caution, vagueness, is not a good working hypothesis for science. I think it must be clear to all of us working in the systematic field that the degree of controversy between structuralists, functionalists, configurationists, behaviorists, etc., is maximally articulate and minimally fruitful. I have tried to focus the discussion on the fundamental assumptions underlying psychology and the study of human be-

havior. To this end I have formulated a set of postulates[5] which are short and at the same time as inclusive as I have been able to make them. Of course I realize their shortcomings, but as a beginning they may serve as an outline into which substitutions, alterations, and extensions, can be introduced. With a set of postulates for each of the divisions of psychology, or for individual psychologists, we can place before each other and our students something tangible upon which the 'survival of the fittest' may operate with greater expedition. *

Ohio State University ALBERT P. WEISS

AN ANSWER TO DR. BORING

In the April, 1925, number of the JOURNAL, appears a note by E. G. Boring entitled "Scientific Induction and Statistics," in which articles by Murchison and Gilbert,[1] and by Murchison and Pooler[2] are attacked as if in a casual reply to Kelley.[3] The two articles in question were written by graduate students of mine, and the data contained therein have never up to now been checked by me. Since I did check the conclusions in the two articles in question before signing my name as joint author, I shall attempt a brief reply which will add perspective to the principles involved.

(1) The two articles in question by Gilbert, Pooler, and Murchison are but two chapters in a very large book to appear soon. When this material first began to be published in the *Journal of Criminal Law and Criminology*, all of this was explained very carefully. In every succeeding article, reference is made to this first publication.

(2) The method I have followed in presenting my data in all my publications on criminal intelligence was decided upon after mature deliberation and consultation with able statisticians. The method makes possible complete fusion with future data, and is the only general method which makes such future amalgamation possible. The method consists in giving simple arrays of quantitative measurements, with the discussion strictly limited by the data presented. All conclusions are headed 'tentative,' and are to be revised in the light of future accumulations of data. I have in my possession two letters from world famous statisticians congratulating me on this simple, lucid, and useful method.

(3) The method of presenting averages, deviations, errors, and probabilities is a method that is not useful in presenting a large number of relatively small groups of quantitative measurements. I am dealing with not more than 7000 cases in all, and these cases have been classified into a very large number of small groups. I discussed this matter at much length in the *Journal of Criminal Law and Criminology*,[4] and actually presented without comment ten pages of closely printed lists of averages, deviations, errors, correlations, and regression coefficients. It was obvious to me that my groups were not large enough for such methods to be used with profit, so I presented such figures without comment and proceeded to use the method of simple arrays of measurements.

(4) Boring announces that "the thing to do is to compute the average scores and their probable errors and then to compute the constants that

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[5] *Op. cit.*, Chapter XVII.

* [A rejoinder by Dr. A. E. Davies will appear in the October number of the JOURNAL —Ed.]

[1] C. Murchison and R. Gilbert, Some marital concomitants of negro men criminals. *Ped. Sem.*, 32, 1925, 652-656.

[2] C. Murchison and Priscilla Pooler, Length of incarceration and mental test scores of negro men criminals, *ibid.*, 32, 1925, 657-658.

[3] T. L. Kelley, Principles and technique of mental measurements, this JOURNAL, 34, 1923, 408-432.

[4] C. Murchison, American white criminal intelligence, *J. Crimin. Law & Crim.*, 15, 1924, 239-316, 435-494.

bear upon the 'significance' of the difference." This is the method I discarded as worthless in the very beginning. It is a serious matter for a man in one field of psychology to assume to teach a man in another field of psychology how to play his game. Such helpfulness is meritorious, and should be appreciated if it is valid and can be trusted. Personally, I am seeking advice all the time from all sources that can be trusted. Can I trust Boring's advice in this matter? He computes for my inspection the averages, differences, errors, etc. which he says I should have computed. Whether I should or not I considered and decided against years ago. But can I trust Boring? Even though I realize that he does not know even in an elementary way the problems I am dealing with, can I have confidence in what he does know? On the basis of what data was he able to compute the above averages, differences, errors, etc.? The raw original data have never been published and have never gone out of my possession. How did Boring make his computations? He simply assumed that all the measurements in a class-interval are compacted at the centre of that interval. Such assumption is statistically proper in the treatment of large groups, but is sheer nonsense for the analysis of small groups. Furthermore, his figures are all wrong. As an example: he computes the average score of married whites as 80.78, when the true average is 74.3. His computed average deviates from the true average twice the probable error of the average.

(5) Is Boring competent to engage in this type of criticism? On the first page of his Note, he makes the following statement: "We come certainly to induction when we wish to assert that all A's tend to be greater than all B's, that is to say, any group of A's will on the average be greater than any group of B's." Boring here links with the words "that is to say" what he supposes to be two logically identical statements. To pose as a teacher of logic, and blunder like that! On the second page of his Note, in discussing my method of presentation of data, he makes the following statement: "I wondered why two pairs of such similar distributions were printed side by side if not for the sake of demonstrating a difference." Boring's assumption that it must be for the sake of demonstrating a difference is most amazing!

(6) Does Boring, in his own research work, observe the principles he is attempting to demonstrate to me? On the second page of his Note, in discussing Gilbert's cautious and valid tentative conclusion "on the average, married negro criminals seem somewhat less intelligent than the unmarried," he offers the following substitution: "The adequate statement would be that criminals were thus and thus in that particular prison at that time, and that they do not, when statistical insight is had into the case, even seem to be thus and thus in general." The 'statistical insight' that Boring mentions is an animal I have never met. But wholly apart from that, to what extent does Boring in his own research publication state his conclusions wholly in terms of his data in their time and place? After his investigation of appetite,[5] based only on *three* subjects, he stated the following conclusion among others: "The most constant and characteristic feature of appetite is an oral, deglutitory attitude." Boring here makes a universal conclusion on the basis of data from three subjects. After investigating the question of introspection in dementia precox,[6] using only *eight* subjects, he makes the following general conclusion: "Persons with dementia precox can, under experimental conditions and without prolonged special training, give reports indicating the general trend of consciousness." He should have said 'some persons,' but did not mean that at all. After investigating the sensitivity of the alimentary canal,[7] using only *seven* subjects, he made the following general conclusion: "The esophagus is

[5] E. G. Boring and Amy Luce, The psychological basis of appetite, this JOURNAL, 28 1917, 443-453.

[6] Introspection in Dementia Precox, this JOURNAL, 24, 1913, 145-170.

[7] Sensations of the alimentary canal, *ibid.*, 26, 1915, 1-57.

sensitive to warm and to cold stimulation throughout its length." In making the conclusions from this investigation of seven subjects, Boring formulates more than *sixty universal generalizations!* All of Boring's scientific contributions would need either to be thrown overboard or entirely revamped if he were to follow the principle which he is trying to teach me.

(7) Kelley is the one statistician who has taken the trouble to discuss Boring at some length. What does Kelley think of Boring's ideas in the general field of statistics? In discussing a systematic statement by Boring, Kelley says: "The procedure described is such as I believe a competent scientist never resorts to. . . . 'Absurd' hardly characterizes this procedure. Is it not rather a matter of the honesty of the investigator?"[8] "As a consequence of the foregoing I contend that the limitations which Boring ascribes to much of statistical procedure are unwarranted."[9] "Our conclusion therefore is that none of the measures advocated by Boring is free of those things which he inveighs against."[10]

Yet Boring assumes that he has found something which gives him an opportunity to show that both he and Kelley are right![11] It is exceedingly probable that Kelley is right.

Clark University CARL MURCHISON

THE 'VISUAL PERCEPTION' OF MOVEMENT *vs.* THE 'APPREHENSION' OF MOVEMENT FROM VISUAL STIMULI

Once more the problem of visual movement has been attacked,[1] and this time with the stated purpose of checking up the results of earlier investigations (75).[2] Inasmuch as we come in for a fair share of the discussion, it will not be amiss to consider the new work in the light of our own experiments. When an author presents a set of results in terms of which he proposes to criticise an earlier investigation—especially when that criticism is adverse—we have a right to demand that his experimental setting and procedure be comparable with that of the work criticised.

The relevance, then, of Higginson's objections, which are based upon his inability to correlate temporal interval or 'grey flash' with optimal movement, depends entirely upon the completeness with which he duplicated our conditions—the necessary conditions for such a correlation. In some ways Higginson's experiment is similar to ours; but the differences are so fundamental as to preclude the possibility that he could duplicate our results.

(*a*) In the first place, Higginson's stimulus is entirely different; and the difference unquestionably introduces factors that were not present in our work. He admits that the irradiation of the light about the slits might have been reduced (75, note 17). Such irradiation of a light, as it is turned on or off, very readily gives the impression of movement. Just how Higginson's substitution of light directly transmitted through a slit for an uniformly illuminated field in which dark rectangles were set gives "a greater control over the extra-organic factors than has been obtained in most of the previous investigations" (75) is not apparent. Certainly, it precludes any direct comparison of his results with our own.

(*b*) The *Aufgabe* under which Higginson's *O*s worked differs materially from ours. We called for 'description' or 'characterization,' as the case might be, and demanded complete assurance (*319f.*). This task was

[8]Kelley, *op. cit.*, 411. [9]*Ibid.*, 411. [10]*Ibid.*, 418.

[11]Boring, Scientific induction and statistics, this JOURNAL, 37, 1926, 304.

[1]G. V. Higginson, The visual apprehension of movement under successive retina excitations, this JOURNAL, 37, 1926, 63-115.

[2]Page references in roman type are to Higginson's article; those in italics are to our paper, An experimental study of visual movement and the Phi phenomenon, this JOURNAL, 31, 1920, 317-332.

adequately performed after a fair amount of practice. All *Os* attained to stability of report; and no reports were acceptable until they became stable. In contrast, Higginson's directions to "observe carefully whatever comes and report," and to "indicate by the number 1, 2, 3, or 4 the degree of assurance" (78) were, he admits (79), "ambiguous" to his *Os*; and he endeavored to correct the ambiguity by further directions as to what they should and should not report. The *instability* of the reports under these instructions is a point of which Higginson makes a great deal (79, note 21; 83, 105, 111). The disagreement of two groups of *Os* under two so different sets of instructions is only to be expected.

(*c*) Higginson's bases of interpretation and use of terms are so different from ours that an agreement of interpretation would be impossible even if the results were identical. Higginson's *Os* 'apprehended' movement; and although in his title he calls this movement 'visual,' it turns out that it may be carried auditorily, tactually, or kinaesthetically (98, 103, 110). This sort of apprehension of movement from visual stimuli is not what is ordinarily meant by the perception of *visual* movement. Moreover, Higginson says: "It must not be assumed that 'full-movement' always meant that a line was apprehended to move, as a line, across the field; *although this is the usual meaning of the term in previous studies.*[1] The *Os* instead reported at times that the experience *meant* that one line moved from one position to another; but that the actual movement of the line across the field intermediate to the two perceived positions was not seen" (79). But we defined "optimal movement" as "the movement of a line all the way from one position to the other" (*318*); and this movement had to be perceived visually, *i.e.* seen, and could not be simply *meant*.

Higginson's attempt to apply his criteria to our results has led him into a number of detailed criticisms that are purely artificial and that lose all force when considered in their proper setting. Not one of them will bear scrutiny. We give a few examples. (1) "It is strange that he [Dimmick] does not cite figures or other testimony to the alleged change of optimal over into unimembral and bimembral movement as he passes temporally in either direction from the optimal interval" (101). It would be strange had we not done so! Tables I and III of our report (*321f.*) furnish the desired data. We were not particularly concerned with partial movements (see the statement of our problem, *317*); but we have given the essential facts concerning them that came out of our work. (2) "We must examine certain of his [Dimmick's] 'movement' reports which seem to us ambiguous" (105). Higginson quotes 11 of our reports, and shows that he can interpret them otherwise than we did. But he uses his own definition of the 'apprehension' of 'full-movement'—although he knows that it is different from our 'perception' of 'optimal movement.' Our reports are, we think, entirely straight-forward—if they are read with a proper regard for our definitions. (3) "As we know, Dimmick stands for a close correlation between 'process' configuration and type of movement" (107). Higginson copies some reports from our paper and remarks that "these reports are cited by Dimmick as representative of the alleged correlation" (108). Higginson has the cart before the horse. We did not "stand for a close correlation" until we had examined our results. Nor is it true that "these reports are cited by Dimmick as representative of the alleged correlation." Instead, they are *samples* which show "*the nature of the visual experience which filled the interval*" (*323*). (4) "If, under the process instruction, *Os* generally observed only 'process,' there would seem to be neither logical nor experiential justification for claiming that the process configuration could possibly mean anything more than just what it was" (109). Here and in the remainder of the paragraph Higginson is not clear regarding

[1] The italics are ours.

the distinction between 'description of process' and 'statement of meaning.' His remarks about the numerical interpretation seem also to imply a misunderstanding. The values he quotes are obviously parallel, although he has chosen the poorest but one of the 16 sets given. What can he mean in this connection by the statement, "What the organism apprehends at one moment is not to be identified with its earlier and later experiences" (109)? Further, what does his table of differences "which should not be overlooked" show? It appears to show nothing more than that we have dealt with a finite number of cases. (5) "If the field were filled *uniformly* with a grey (to condition full-movement) . . . ." "the reader gains the impression that a plain but brief grey was being described;" "we have little or no description of the temporal aspect" (110). Again we appeal to our original exposition. We never characterized the 'grey flash' as uniform; we called it *multiform*. We described the temporal aspect as *brief*. What further descriptive terms are there that are applicable to this duration? The integration of the two attributes gives a "shimmery," "unsteady," "live," liquid," "flame-like" grey.

Such criticism is no criticism at all. The ambiguities, discrepancies, etc. that Higginson remarks do not exist in our presentation of results and conclusions. On the other hand, he seems to have shut his eyes to the aspects of his own results which, in spite of the different conditions of his experimental setting, confirm the results of our earlier study.

Summary. Our reply, then, to Higginson's criticisms is categorical. (1) His experimental setting is entirely different from ours, and therefore can offer no basis for criticism of our results. (2) His problem, as indicated by his use of the term 'apprehension,' is also different. (3) His objections to our presentation of results are based upon misinterpretations of, and disregard for, our explicit statements.

Hobart College FORREST L. DIMMICK

MEETING OF THE MID-WESTERN EXPERIMENTAL PSYCHOLOGISTS

After a lapse of twenty years or more a group of mid-western experimental psychologists were invited to an informal conference at Northwestern University, Evanston, Illinois, on Friday and Saturday, May 7 and 8. The meeting place was appropriate because several decades ago, meetings were frequently held at Evanston and Chicago which were attended by such distinguished psychologists as Angell, Baird, Colvin, Max Meyer, Scott, and Watson. Experimental psychology certainly showed its vigor when after this interval more than 100 people interested in psychological research would come together and discuss current problems.

The sessions were opened Friday evening by an address from Jastrow, who surveyed the present field of experimental psychology and incidentally lamented the appearance before the public of fake psychologists and pseudo-psychologies. He indicated a parallel phenomenon in the history of medical science and practice and felt sure that as soon as the public became more educated in the matter they would distinguish the true from the false. He outlined the development of mental hygiene as a distinct American contribution and made some reference to the increasing need for the control of the emotional life which required a more intensive experimental study of human feelings.

On Saturday morning and afternoon the sessions were held in the lounge room of the Patten gymnasium. Gilliland and Morgan (Northwestern) presided at the morning sessions and E. S. Robinson (Chicago) in the afternoon. Both sessions were attended by about 75 to 100 persons. There were a number of prepared reports in the morning and one in the afternoon. Vaughn (Chicago) gave the results of an experiment on learning under negative incentive following the work of Rexroad and others on

the effect of punishment in the learning of human individuals. He has found in general that, while there is a large individual variation especially in emotional attitude, toward the experiment, on the whole the rate of movement is slower and attention is keener in the series with threatened punishment. The effect of the threats has not been fully investigated but it is apparently not pronounced except for a short period of time. Positive incentive in terms of reward was less effective in learning than negative incentive, in the way of punishment, but the positive incentive was better than no incentive at all.

H. M. Johnson (Mellon Institute) gave an account of some experiments on restlessness during sleep. Ten *O*s slept on beds which were supported by four very sensitive pendulums in each case. At 5 min. intervals throughout the sleeping time of approximately 7¾ hours, records were automatically taken of the amount of motion produced by the sleeping person. The sudden movement of an arm was sufficient to be recorded. In general the most active *O* stirred once in every 1¾ min. and the least active *O* stirred on the average of once every 16 min. The mean for both groups was 11.7 min. Further investigations were made on the question of mental capacity before and after sleep. Two forms of substitution tests were used one-half before hour and one-half hour after sleeping. In one series the evening performance was on the whole 10% better than the morning performance and in another series 6% better. These results confirmed other reports made in industrial investigations. They are explained in one of three ways: (1) that the toxic effects of fatigue may indeed act as a stimulant before retiring; (2) that in the morning the habit of relaxation continues into the waking period; (3) that there is a large individual variation and a scattering of activity through the night which must be taken into account. Some individuals moved most during the first quarter and less during the next two quarters, while the activity of others was distributed in various patterns during the night.

S. L. Stevens (Northwestern) attempted to correlate objective response with conscious behavior on the theory of what he called *metabolic functionalism*. The experiment used a compound reaction device with five handles numbered from 1 to 5 and two pedals lettered A and B. The instructions required a quick response to a complex pattern. Any experimental series could be interrupted by *E* or *O* for an introspective report. Three problems were investigated: (1) the process surrounding the perceptual clearing up of the stimulus; (2) the conscious adjustment to the stimulus in terms of meaning; (3) the function of attention as a conscious process. Changes of bodily expression, especially the shifting of eyes, as a cue to attention were noticed by *E*. No definite results were at hand but the data so far indicated that conscious behavior is a unique phenomenon and that it is more of a 'doing' than of a 'having.'

C. R. Griffith (Illinois) outlined his experiments in connection with athletics. They included investigations of the memory span for such materials as football signals and the various characteristic performances of each of 9 baseball players while 'at bat.' The rate of memorizing is also being investigated. Other researches go into the problem of motor adjustment under situations analogous to each one of the sports, some of which were in the form of serial reactions. It has been found that many men incapable of "making a basket" in basketball do better when the motor mechanism is first automatized under blindfold conditions. On the whole these tests so far devised tally with the coaches' estimates to the extent of indicating the highest 10% and the lowest 10% in the varsity squads. On the side of pure psychology these investigations show that motor learning may mean not merely the acquisition of motor skill but an acquisition of motor dissociation so that one part of the body will react quite independently of another. Again, in terms of the more recent dis-

cussion of *Gestalt*, the problem of integrating perceptual and motor responses in a single pattern was brought up. An expert football player in an uncanny way would be able to judge that an opening in a line was about to be made. He would be able successfully to perceive a situation in the process of its formation.

Max Meyer (Missouri) discussed a paper on the application of psychology to industry. He objected to the traditional use of reaction times which involves single jerky movements not commonly found in the industries. On the other hand he objected to too great a complexity of apparatus which presented conditions too closely analogous to the situation to be investigated. Between these extremes lies the device which he has perfected on the basis of the well known 'Cartisian diver.' There are two divers in two separate jars, each one controlled by a separate lever in the right and left hands. The problem was to keep the diver midway between the top and bottom of the jar in rhythm with the sounding of bells. A time clock would indicate when mistakes had been made and the aim of the *O* is to keep on pumping in alternate directions in rhythm with the bells by varying the intensity of the strokes. It is inferred that a leak in the system makes it necessary to keep up a certain amount of pressure.

Hull (Wisconsin) began the afternoon session with a discussion of some experiments performed under hypnosis. (1) Post-hypnotic amnesia was studied with nonsense material and by means of several of the classical methods. Individuals showed a 100% recognition of material from one trance state to another but could not recall any of the material in the waking state. Relearning the material during the waking period, however showed 50% greater efficiency than would have been possible without previous learning. (2) The strength of post-hypnotic suggestion in which an *O* is told to do something in the waking state was also studied. The suggestion was made that he would keep breathing slowly when he read odd pages of the book and faster when he came to the even pages. The rate of disappearance of this suggestion was carefully studied and so far results show that the lasting effect is greater than the waning effect during the first few hours.

When the informal laboratory reports were called for it was found that there was not enough time to cover all laboratories represented. The order of report was then determined by chance and the first lot fell to Ohio State. Renshaw discussed human posture both phasic and tonic, and especially the reciprocal relationship between these. He briefly outlined the segmental postures like the accommodation of the eyes and he discussed individual differences in maintaining postures in long and tedious mental tasks. The hydrogen ion concentration of the saliva was used as the best indicator of vascular and visceral disturbances as well as an index for general metabolic acidity of tissue. On the basis of these studies he classified individuals into four groups: (1) those who are metabolically quiet during the no-task period and active during work; (2) those who are quiet during the no-task period and quiet during work; (3) those who are active in the no-task period and active during work; (4) and those who are active during the no-task period and quiet during work. Rexroad continuing his work with negative incentive reported investigations bearing on the relation of verbal response to finger response in a multiple reaction device. He encountered several curious results. One concerned a double thumb movement which would necessarily be wrong but which showed a sort of perseveration or automatic response the second time. He also showed that *Os* had a tendency to respond to the word green when visually displayed in place of the color green. Even when the other fingers were tied back 29 out of 32 *Os* still responded to the word green and 11 out of 32 responded to the word green when auditorially presented, even when they knew that this was a wrong response.

Dockeray (Ohio Wesleyan) reported some experiments on the phantom sound with binaural beats through the use of the oscillating circuit. His results are uncertain as yet. In another direction he found that the conduction of sound through an intermediate *O*'s head was impossible except with the sound of the spoken voice.

Ruckmick (Iowa) was requested at this point to discuss experiments on sound localization which had just been completed. He emphasized the desirable factors of the heterodyne circuit which produced relatively pure tones without electric filters. A series of investigations gave unequivocal results for phase differences between the lowest audible tone and a tone of approximately 8,000 cycles. He outlined also a published investigation which showed that intensity is a deciding factor in the middle range of tone values.

Reymert (Wittenberg College) reported on some experiments investigating kinaesthetic sets with ball tossing at targets by means of the worse arm. At the same time the psychometric function as related to the attensiometer was calculated. This research also used the Whipple target, the rifle aiming response and muscular strength and rhythm. He outlined further some experiments correlating quantitative scores with qualitative introspective reports. Materials so far used comprise the Seashore musical tests, the Healy pictorial tests and other means. Experiments were also directed on the problem of meaning with geometrical designs using three-year old children as *O*s. A preponderance of kinaesthetic cues was found.

Wheeler (Kansas) showed some tentative results on neuromuscular rhythm. If an *O* showed a decided decrease in performance on the 20th circuit in the maze he would almost always show similar decreases on the 40th, 60th, etc., turn. But so far there have been two groups of *O*s: those who show (1) high steeples or crests at certain periods; (2) those who show relatively low steeples. Heron has been continuing some experiments of Hunter on the conditioned reflex with the tilting of the learning frames in which white rats were subjects and, also, on the problem of orientation in the horizontal plane. So far he has results which show that the white rat does not keep on the same side on which he has turned from his successful performance. He is also experimenting with a frame constructed of 4 tubes in the shape of a cross which can be tilted up or down so a rat going up one tube for food will be compelled to go down the same tube at another time. So far the results are not well established as regards the effect of tilting in this way.

At this point there was an intermission during which refreshments were served. The session was then continued by Jacobson (Minnesota) who showed that there were no effects in skilled performance after the extirpation of the frontal lobes, and in a few cases of the cerebellum, in some of the lower animals including rats and chimpanzees. Metfessel (Iowa) who reported the natural fluctuations of the voice in the (a) vibrato, (b) the tremulo, (c) the emotional changes with laughter and grief, (d) the trill and, (e) fast fluctuations of pitch and intensity superimposed on any of these. Through a refined method of amplification photographic records of these changes of rate are recorded. Travis outlined experiments in neuromuscular action-currents with a 40,000 cycle oscillator and two stages of audio-frequency amplification. This wave is received through an 8-tube superheterodyne radio set and finally recorded through a phoneloscope. The speed of conduction from each hemisphere to the two arms is the problem under investigation as well as the action of the recti in abdominal breathing.

In the evening over 70 persons attended the banquet at the Orrington Hotel. President Scott (Northwestern) was master of ceremonies. He gave an historical account of the development of the campus and of meetings formerly held at Northwestern and outlined the future of experimental

psychology. It is already impossible for any one man to know the whole field thoroughly and the demand for young psychologists appears at present to be from year to year greater than the supply. He noted incidentally that pure psychology must guard itself against two tendencies: (1) that of moral misinterpretation on the part of the public, and (2) the encroachments made by industry on our personnel. Already two or three distinguished psychologists have become vice-presidents of large corporations and even universities are making administrative demands on professional psychologists. The presidents of six of the larger midwestern universities have been drawn from the ranks of psychology. Carr (Chicago) was the next speaker. He outlined the reasons for the disproportion between the strength of stimulus and intensity of sensation in the Weber-Fechner law. He was inclined to put the difference in terms of a mechanical loss. After briefly outlining the various historical interpretations in terms of loss in nerve conduction at the synapses or at the neural center or in the brain or in the outside media, he came to the conclusion that there never was a one to one relationship between cause and effect in any physical phenomenon without loss of some kind of energy, or a transformation of energy into other forms.

Weiss (Ohio State) outlined the experimental program at his laboratory which concerns itself chiefly with the biosociological approaches to genetic psychology in three ways; (1) how does the child get to be an adult? *i.e.* what are the mental and physical conditions surrounding each stage of development, (2) how does a child get to be a child? *i.e.* what are the inherent forces that develop within from inherited traits, and (3) how does a child get his environment? *i.e.* to what extent does the environment influence him and to what extent do he and others influence his environment.

Several invitations were received for next year's meeting and Gilliland was authorized to act as chairman of the committee[1] which will make the necessary decisions and arrangements. At the meetings and banquet there were representatives from psychological laboratories at the following institutions: The universities of Chicago, Illinois, Iowa, Kansas, Minnesota, Missouri, Northwestern, Ohio State, Ohio Wesleyan, Wisconsin; Chicago Normal College, Lake Forest, Wittenberg, and Mellon Institute. Many psychologists independently commented on the amount of free discussion that accompanied each paper and informal report and they expressed a keen desire to have this conference become an annual event.

University of Iowa C. R. RUCKMICK

THE TWENTY-THIRD ANNUAL MEETING OF EXPERIMENTAL PSYCHOLOGISTS

The twenty-third annual meeting of Experimental Psychologists was held in the psychological laboratory of the University of Pennsylvania on April 5-7. Professor Samuel W. Fernberger acted as host and chairman. Eleven laboratories were represented. Members of the faculty and graduate students were in attendance from the University of Chicago, Columbia University, Cornell University, Harvard University, Hobart College, Princeton University, Wittenberg College, Wellesley College, Wesleyan University, and Yale University.

The opening meeting on the afternoon of April 5 was, in commemoration of the 125 anniversary of the birth of Gustav Theodor Fechner, devoted to a discussion of psychophysics. The other meetings were given over to reports from the various laboratories of work that had recently been completed or that was still in progress.

[1]The personnel of the committee consists of Professors A. R. Gilliland (Northwestern) chairman, C. R. Griffith (Illinois), C. Hull (Wisconsin), E. S. Robinson (Chicago), A. P. Weiss (Ohio State), and C. A. Ruckmick (Iowa).

At the close of the evening meeting on April 6 the following resolution, proposed by Professor E. B. Titchener, was unanimously adopted: "Resolved, that this meeting deplores the increasing practice of collecting administrative or supposedly scientific data by way of questionaries; and that the meeting deplores especially the practice under which graduate students undertake research by sending questionaries to professional psychologists."

K. M. D.

The Journal of Philosophical Studies

We have just received the first number of the *Journal of Philosophical Studies* (Vol. 1, No. 1, dated January 1926) which is published quarterly by the British Institute of Philosophical Studies.

The *Journal* is edited by Sydney E. Hooper with the assistance of William Brown, M. Ginsberg, G. Dawes Hicks, L. T. Hobhouse, Julian S. Huxley, F. B. Jevons, Harold J. Laski, A. D. Lindsay, J. H. Muirhead, Bertrand Russell, G. F. Stout, A. E. Taylor, J. Arthur Thomson, and Clement C. J. Webb. It was founded as a means of keeping the widely extending membership in touch with the activities of the Institute, and also in the hope that the object of the Institute—the advancement of philosophical studies, "the supreme desideratum of our age"—would in some small measure be furthered.

As explained in the prefatory editorial, the *Journal*—since only a small minority of its readers have been professionally trained in philosophy—will "avoid being too severely technical" and will "aim at making philosophy intelligible to the well-informed student." Besides articles of general interest, it will publish original contributions which may be of special interest only to the expert, summaries of different fields of philosophy, surveys of philosophical literature in the various languages, reviews of books and important articles, and notes and news of the Institute.

This number, after the Editorial, contains the following articles: S. Alexander, Art and Science; G. Dawes Hicks, The Metaphysical Systems of F. H. Bradley and James Ward; Morris Ginsburg, Emotion and Instinct; J. Arthur Thomson, Different Kinds of Evolution; John S. Mackenzie, The Present Outlook in Social Philosophy; and F. B. Jevons, The Purpose of Philosophy; abstracts of Bertrand Russell's lecture on Perception and T. H. Pear's lecture on The Definition and Scope of Psychology; surveys of recent English Books, of The Doctrines of M. Émile Meyerson, and of Philosophy in Russia; and departments devoted to New Books, Periodicals, Discussion, and Institute Notes. The number closes with a list of officers of the Institute and of the original members of the Council.

K. M. D.